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THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES, AND COMMERCE,

BY ALEXANDER TILLOCH, LL.D.
[illegible]
[illegible]
[illegible] SCIENCES, [illegible] &c.

[illegible] sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes. Just. Lips. Monit. Polit. lib. i. cap. 1.

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THE

PHILOSOPHICAL MAGAZINE
AND JOURNAL.

I. *On the Mean Density of the Earth.* By *Dr.* CHARLES HUTTON, *F.R.S.*

ALTHOUGH the determination of the mean density of the whole terraqueous globe of our planet, is admitted to be a problem of the utmost importance to several branches of philosophy, particularly to physical astronomy, and the figure and constitution of the earth; it would seem, from the discordancy of the declared opinions of some eminent philosophers, that the problem is still in an uncertain state. Since the first notice of this subject by Newton in his admirable *Principia*, it has often been incidentally alluded to without receiving a precise determination, with the exception of two instances only, in which it has been stated to be certainly or approximately determined by experiment, namely, in the case of the Schihallien experiment, by Dr. Maskelyne and myself; and by the Hon. Henry Cavendish, by a method invented by Mr. Michell.

The former of these experiments was made by Dr. Maskelyne in the years 1774, 1775 and 1776, by means of that large mountain in Scotland, in measuring its dimensions, and in comparing its attraction on a plummet, with that of the whole earth on the same; the calculations on the same being made by myself, and first published in the Philos. Trans. of the year 1778; and since more correctly in the 2d volume of my Mathematical Tracts. The other experiment by Mr. Cavendish, was by observing the attraction on small pendulous balls, of two inches diameter, by larger ones of ten inches diameter, as compared with the attraction of the earth on the same.

By some strange mistake, or perversion, for many years, it was customary among certain persons, to withhold the mention of my name, with regard to the great share that I had in the experiment on Schihallien. But from certain complaints which I have made, some little justice has lately been awarded to me on that head; though still it would seem with reluctance, as the opinion is promptly assumed that the latter small experiment is susceptible of the greater accuracy, and the numbers in its result gratuitously adopted as nearer the truth than that of the former. As this is an opinion which I have never been able to bring my mind to acknowledge; and as it is a matter of great importance in the present state of physics, I have been desirous to draw the attention of philosophers to a closer consideration of the subject; with a view to a more deliberate and impartial decision of this point.

From the closest and most scrupulous attention I can employ on this question, the preference, in point of accuracy, appears to be decidedly in favour of the large or mountain experiment, over that of the small balls. It is indeed true, that though the large mass of the mountain must yield an immensely greater force than a small ball; yet it may be said that this advantage must be balanced, either wholly or in great part, on the score of distance, as the plummet is acted on at a great distance from the centre of the mountain, while the balls are approached very near together: so that the visible effects may thus lie nearly equal, by the reciprocal balancing between magnitude and distance. Hence the visible effect of the mountain, is that of the small angle of 11 or 12 seconds, by which the plummet is drawn aside from the perpendicular; thereby showing that the attraction of the earth, on the plummet, is to that of the mountain on it, as radius is to the tangent of those seconds; while, in the other experiment, the small pendulous balls are drawn aside by the large ones the space of between $\frac{1}{7}$ and [illegible] of an inch; the distance of each ball from the middle of their connecting rod being a little more than 36 inches. The first or immediate small results of the two experiments, thus appearing so far to be about equally favourable, it will be necessary to examine the circumstances of each of them separately, that we may be able to judge more particularly of their merits; and first, of the Schihallien experiment.

This experiment, it is well known, was conducted by the late astronomer royal, Dr. Maskelyne, than whom a more correct, faithful and experienced individual probably never existed. The accounts of his measures and observations, taken in conducting it, are minutely detailed in the Philosophical Transactions of the year 1775, or in my edition of the Transactions, vol. xiii. p. 702; where all the instruments and operations are particularly described in the most plain and satisfactory manner. The principal instrument was the ten-foot zenith sector; with which the meridian zenith distances of 43 stars, by 337 observations, were carefully taken, both on the north and south sides of the mountain. The medium of all these, with other necessary measures, gave a final result of 11·6 seconds, for the sum of the deviations of the plumb line, on both sides of the mountain; and that in all probability within much less than half a second of the truth. Other instruments used, were the Royal Society's transit instrument made by Bird, and an astronomical clock by Shelton, which had both been provided on occasion of the observations on the transit of Venus, in 1761 or 1769. Besides these and several other instruments, one of Ramsden's best theodolites was used, in measuring the figure and dimensions of the mountain, which was performed in the most correct manner by skilful surveyors; so as that thence an exact model of it might be made, or all its dimensions accurately taken, for computing the attraction

By only reading over the accounts of these operations (in the places before mentioned) made by means of such instruments, and in such hands, every person must be convinced of the impossibility almost that any error could have been committed, capable of causing any sensible inaccuracy in the conclusion of the work.

It remains now to describe the share which I bore in this important business; which consisted in taking all the measurements as above described, and from those data, calculating what must have been the exact magnitude of the mountain, what its attraction on the plummet, relatively to that of the globe of the earth on the same, and what must be the mean density of the earth. These computations, which employed my daily and assiduous labours during the greater part of two years, are recorded in the Philosophical Transactions of the year 1778, and also in the 2d volume of my Mathematical Tracts. It may there be seen that, after computing trigonometrically the bearing and distance of every point in the numerous sections of the mountain, from the two observatories, I conceived it to be divided into nearly one thousand vertical columns, of given bases and altitudes. I then computed the quantity of the attraction of all these columns, on the plummet, in the direction of the meridian, when placed at the two observatories, on both sides of the hill, where the whole effect had been observed, which attraction was thus found to be expressed by the number $8811\frac{5}{6}$. I then computed, from the magnitude of the earth, what must be its attraction on the same plummet, and found it expressed by the number 8752720. Consequently, the whole attraction of the earth, is to the sum of the two contrary attractions of the mountain, as the number 8752720 to $8811\frac{5}{6}$, that is, as 993·3 to 1 very nearly; on supposition that the density of the matter in the hill, is equal to the mean density of that in the earth.

But Dr. Maskelyne found by his observations, that the sum of the deviations of the plumb line, produced by the two contrary attractions, was 11·6 seconds. Hence then it is inferred, that the attraction of the earth, is actually to the sum of the attractions of the hill, nearly as radius to the tangent of 11·6 seconds; that is, as 1 to ·000056239, or as 17781 to 1; or as 17804 to 1 nearly, after allowing for the centrifugal force arising from the rotation of the earth about its axis.

Having now obtained the two results, namely, that which arises from the actual observations, and that due to the computation on the supposition of an equal density in the two bodies, the two ratios compared, must give the ratio of their densities, and which therefore is that of 17804 to 9933, or 1434 to 800 nearly, or almost as 9 to 5; and so much does the mean density of the earth exceed that of the hill. Consequently, if we knew the density of the latter, we shall thence obtain that of the former.

At the time when this computation was first printed, in the year 1778, the real density of the hill was unknown. It was only known that it consisted chiefly of very hard and dense rocks, much heavier than common stone, which is allowed to be $2\frac{1}{2}$ times the density of water. I then, by way of example in applying the density, multiplied $\frac{9}{5}$ by $2\frac{1}{2}$, which produced $\frac{9}{2}$ or $4\frac{1}{2}$ for the density of the earth, on the smallest assumption; till such time as we should come to know more nearly what the real density of those rocks is: and therefore I must feel reason to complain, that this number ($4\frac{1}{2}$) has often been stated, rather unfairly, as my final conclusion for the earth's mean density; instead of being only the very lowest limit that might be used, till we could better learn something on that point with more certainty. But, a lithological survey of the mountain being afterwards accurately made at my earnest request, by that excellent philosopher and geologist Mr. Playfair, the result of which was published in the Philos. Trans. for the year 1811; I then applied his mean statement of the rocks to my own calculations, which gave me the number 5 for the density of the earth; as I published in the 14th volume of my edition of the Phil. Transactions, and in the 2d volume of my Tracts.

In Mr. Playfair's account of the mountain, are given the names and nature of the several rocks that compose it, with tables or lists of their densities or specific gravities. In one table is a list of thirteen specimens of densities, contained between the numbers 2·6109 and 2·6656, the medium of the whole being 2·6[illegible]. In another table, of fifteen specimens, the densities are limited between 2·7185 and 3·0642, the medium of all which is 2·8[illegible]. And the mean between these two means, gives 2·725 for the mean density of the whole mountain, admitting it to be quite solid, or without vacuities, as it appears to be on the exterior surface at least. But in the calculation in my Tracts, I went even a little higher, using the number 2·75 or $2\frac{3}{4}$, thus $\frac{9}{5} \times 2\frac{3}{4}$, which gi

$\frac{99}{20}$ or 4·95 for the mean density of the earth. Or, if we assume the density of the mountain still higher, as 2·8 instead of 2·75, we then obtain $\frac{9}{5} \times 2{\cdot}8 = 5{\cdot}05$, a little more than 5 for the earth's density; which last number 5 I therefore fix upon in conclusion, as probably the nearest to the truth; or at least it is sufficiently large, as it is grounded on several assumptions that are most favourable for the highest result; namely, 2·77714 or $2\frac{7}{9}$ for the density of the mountain; also $\frac{9}{5}$ as rather above the calculated ratio of the densities of the earth and mountain; and lastly, the assumption of the mountain being quite solid: though it is probable that there may be cavities in most mountains, as they are generally the production either of volcanoes or of earthquakes.

For all these reasons then, it is highly probable that the earth's mean density is very near five times the density of water; but not higher. If any person should still hesitate to adopt this conclusion, his hesitation must arise from doubts either on the data obtained by the measurements, or on the accuracy of the computations made from them. But if any such person attentively read over Dr. Maskelyne's account of the measurements, in the Phil. Trans. of 1775, his doubts must be soon removed as to the data supplied by the survey of the hill, or by the astronomical observations. And as to the accuracy of my own computations, made from those data, they are fully and fairly before the public, in the works before mentioned; and let any person, who doubts, look over and repeat the calculations there stated, and try if he can find any inaccuracy in them. The only possible ground of doubt in the measured data, must be in the observed deviation in the plumb line taken by Dr. Maskelyne. But when we consider the accuracy of the observer, and of the instruments, and read the account of the use of them, it must be then very difficult to doubt of their accuracy. On the contrary, it is commonly acknowledged that a good observer, with the best instruments, can observe angles to a small fraction of a second! Dr. Maskelyne's observations gave 11·6 seconds for the sum of the deviations of the plumb line, from a medium of between 300 and 400 observations. Now let us suppose it possible to have committed an error of four-tenths of a second in this number, and that the true number should have been 12 seconds, instead of 11·6, being an error of the 29th part of the whole: This then would cause an error of one 29th part in the result; which would reduce the density 5 to about 4·8; showing that the number 11·6 is not too small, but the contrary. Next let us assume 11 seconds only, omitting the 6-10ths, being almost the 20th part of the whole, and which therefore would give nearly 5·25 for the earth's density, being still far below the number 5·48, as deduced from Mr. Cavendish's experiments.

experiment. Hence it appears that our result cannot be made to agree with that of Mr. Cavendish, unless our 11·6 seconds be diminished to about 10·5 or 10·4, on the supposition of an error of more than a whole second in excess, in the number 11·6 seconds; which cannot be admitted without doing great violence to the observations.

Having thus failed in our endeavour to discover any error, or even suspicion of error, in the conduct or result of the Schihallien experiment; let us now turn our attention to the other experiment, as performed by Mr. Cavendish. And here I must at once disclaim all expectation of meeting any failing with regard to the operator himself, whom I well knew to be a most excellent philosopher and mathematician, as well as a patient, accurate, and acute experimenter. The failure then, if any, must be expected from the nature of the machine, and of the calculations.—From the perusal of Mr. Cavendish's account of the machine he employed (in the Phil. Trans. of 1778, or vol. xviii. of my edition), and the nature of the arithmetical calculations, they at once appear to be formidable and discouraging in the highest degree. The machine is small, comparately with those in the former or mountain experiment. It is not easily to be understood, without actually seeing it, though assisted with the view of the drawing of the whole, on account of the intricacy and perplexity of the construction. In the first place, at each end of a light wooden rod of near two yards in length, is attached a small leaden ball of two inches diameter; the middle of the rod being fixed to, and suspended by, a long and very slender copper wire; by any small movement of these balls and the connecting rod, in a horizontal direction, by the torsion or twisting of the wire, a very minute and slow vibratory motion is commenced. To produce this small motion in the two little balls, and their connecting rod, two other large balls, of ten inches diameter, are connected together by certain machinery, at like distance as the former, and capable of being moved to different distances, or positions, on the horizontal level with the [illegible]. By so setting the large balls near the small ones, [illegible] acted by the former, producing a very small motic[illegible] and in consequence a very slow vibration. So min[illegible] motions, that the extent of the vibrations is but a small fraction of an inch, and the duration of each vibration is not performed but in the time of several minutes, from 3 or 4 to near 15 minutes. So minute are these motions, that telescopes and other means are necessary to view and to estimate their quantity and duration. To produce these minute motions, very complex machinery is necessarily employed, while the delicate movements are watched for many hours together, during many days, and recorded with regard to the extent and time of each vibration. Then, from these spaces and times, the density of the earth is to be calculated, by peculiar theorems, as compared with the vibrations of common pendulums, that are produced by the attractions of the earth.

All these effects were so minute, and produced by machinery so complex, and the results calculated by theorems derived from intricate mathematical investigations, that it is impossible at first, for ordinary readers, to conceive how any accurate results can be deduced from them, and even for the more judicious reader to place confidence in them, except chiefly on account of the high character of the experimenter himself. From the nature of the machinery I could therefore derive no confidence in the results, nor compare them with the mountain experiment, without repeating the whole of the calculations. But, after a long life spent in almost daily abstruse investigations, from the tenth year of my age, and now being at 84, and oppressed with distressing illness, I thought I might be excused from such a task. But after urging more than one mathematical friend, without being able to interest them sufficiently to engage in so severe an operation, my anxiety to accomplish the business induced me to make an exertion to effect it myself; especially as the learned experimenter informs us, that he availed himself of the assistance of the then Clerk of the Society, who he says made some of the experiments, and who doubtless made most of the arithmetical computations; operations, of both kinds, in which I remember he was also much employed by Sir Charles Blagden, and other gentlemen, in preparing their papers for the Royal Society. I have therefore re-computed all the exper
s, and have traced the investigations
found that my labour has not been
as been rewarded with the follow-
which are large or important.
o be noted, that the ref
aper, as printed in my edi-
now possessed of a set of
ever, I have had my own

NDISH'S *Paper*.

In page 399, line 10 from the bottom, *for* 8739000, *read* 8740000.

Ditto, line 6 b, or from the bottom, *for* 8739000, *read* 8740000. The same also in line 5 b.

Ib., line 1 b, *for* 10683, *read* 10685. The same also in line 1 b.

Page 403, lines 12 and 13, *for* 8739000, *read* 8740000.

In line 13, *for* 10841, *read* 10847.

Page 103, line 13, *for* 10683, *read* 10685.
Page 104, line 11, *for* 185, *read* 186·5.
Ib. in lines 15, 16, 22, 25, *for* 185, *read* 186·5.

It is to be noted, that after the experiments have been all made, and the motion of the arm carrying the small balls, and expressed in 20ths of an inch, observed and denoted by the letter B; also the time of one vibration expressed in seconds, denoted by the letter N; and both of these being corrected according to certain rules there given; then the mean density of the earth D, in each experiment, is to be computed by this theorem, viz.

$$D = \frac{N^2}{10683\ B}, \text{ or when corrected, } D = \frac{N^2}{10685\ B}.$$

And by that theorem were calculated the following 29 experiments, as they stand recorded in the original.

Table of the Results of the Experiments.

[illegible]	Motion of the Arm.	The same corrected	Time of Vibration	Ditto corrected.	The Density.
	20ths inc.	20ths in.	M. S.		
1	14 32	13 42			5 50
2	14 1	13 17	14 55		5 61
3	15 87	14 69			4 88
4	15 45	14 14	14 42		5 07
5	15 22	13 56	14 39		5 26
6	14 5	13 28	14 54		[illegible]
7	3 1	2·95		6 54	5 36
8	6 18	[illegible]	7 [illegible]		[illegible]
9	5 92	[illegible]	7 [illegible]		
10	5 9	[illegible]			
11	[illegible]	[illegible]			
12	[illegible]	[illegible]			
13	[illegible]	[illegible]			
14	[illegible]				
15	[illegible]				
16	[illegible]				
17	[illegible]				
18	[illegible]				
19					
20	[illegible]				
21	[illegible]		7 [illegible]		[illegible]
22	6 12		7 6		[illegible]
23	5 97		7 7		[illegible]
24	6 27		7 6		[illegible]
25	6 1[illegible]	[illegible]	7 6		[illegible]
26	6 34		7 7		[illegible]
27	6 1		7 16		[illegible]
28	5 78	[illegible]	7 2		[illegible]
29	5 64	[illegible]	7 5		5 [illegible]

The last column shows the numbers for the required density, resulting from the calculation by the foregoing theorem, being all a little above 5, excepting the third number, which is a little below 5. And immediately after, is the following remark, showing the author's doubt of their accuracy; viz. "From this table it appears, that though the experiments agree pretty well together, yet the difference between them, both in the quantity of motion of the arm, and in the time of vibration, is greater than can proceed merely from the error of observation. As to the difference in the motion of the arm, it may very well be accounted for from the current of air produced by the difference of temperature; but whether this can account for the difference in the time of vibration, is doubtful. If the current of air was regular and of the same swiftness in all parts of the vibration of the ball, I think it could not; but as there will most likely be much irregularity in the current, it may very likely be sufficient to account for the difference." It then proceeds: "By a mean of the experiments made with the wire first used," [viz. the first six numbers or experiments] "the density of the earth comes out 5·48 times greater than that of water; and by a mean of those made with the latter wire, it comes out the same." &c.

Now, though the former list of errata were but small in quantity, yet here is one of a considerable magnitude, viz. in the medium of the first six experiments, said to be 5·48, which is very erroneous, the true medium being only 5·31; and it is rather curious that that medium 5·48 has been obtained, by taking the third experiment as 5·88 instead of 4·88, through mere oversight or carelessness. If this were the only error, it might perhaps be excused as a single accident; but the whole will make a very different appearance, when we have shown that many small errors exist in almost all the numbers in the last column of the table, as resulting from erroneous calculations in the use of the general theorem before mentioned, and evinced by a comparison of the numbers in the foregoing table, with those of the following one, derived by our calculation from the same data, and by the same theorem when corrected.

The corrected Table of the Experiment Results.

Experiments	Motion of arm cor.	Time of vibr. cor.	Do. in Seconds.	Densities correct.
	20ths inc	min. sec	seconds.	densities.
1	13·46	14 55	895	5·49
2	13 21	14 55	895	5·59
3	15 17	14 55	895	4·86
4	14 68	14 42	882	4·89
5	14 16	14 39	879	4·93
6	13 63	14 51	894	5·11
7	2 92	6 54	414	5·41
8	3 09	7 1	421	5·29
9	2 96	7 3	423	5·57
10	2 95	7 5	425	5·64
11	2·99	7 5	425	5·57
12	2 85	6 57	417	5·62
13	2·86	6 57	417	5·61
14	2 97	6 57	417	5 40
15	2·95	6 57	417	5 43
16	2 97	6 57	417	5·40
17	2·77	6 57	417	5·79
18	3 16	6 58	418	5·10
19	3·08	6 59	419	5 26
20	3·03	7 1	421	5·38
21	3·05	7 3	423	5 42
22	3 06	7 6	426	5 47
23	2 99	7 7	427	5·64
24	3·14	7 6	426	5 34
25	3 07	7 6	426	5 16
26	3 17	7 7	427	5 30
27	3 05	7 16	422	5 3[illegible]
[illegible]	[illegible]	7 2	422	5 [illegible]8
[illegible]	[illegible]	7 3	423	5 8[illegible]

Here the [illegible] of the first six of these experiments is 5·19; of the other [illegible] it is 5·43; and the mean of both these means is [illegible] 5·48, as stated in the former table, being the [illegible] sum of the numerical calculations. The [illegible] 0·3, about the 17th part of the whole, must [illegible] ascribed to the [illegible] of making and reading off ex[illegible], with such [illegible] and inadequate machinery.

I cannot conclude this paper of inquiry, with[illegible] expressing a hearty wish for a repetition of the large or moun[illegible] experiments in some other favourable situation, and with im[illegible] means, [illegible] possible. For this purpose, I shall venture just to mention an idea which has sometimes occurred to my mind, namely, that [illegible] of the large pyramids in Egypt might profitably be employed, instead of a mountain, for this experiment. Such a body offers several advantages for the purpose. In the first place, the mass is sufficiently large, standing on a base of about the size of the whole space of Lincoln's Inn Fields, and of a height almost double of that of St. Paul's steeple, or near three times the height of the Monument: then the station for the plummet, or zenith sector, could be taken much nearer the centre of the mass than on a mountain, which would give a larger quantity of deviation of the plummet; then the regular figure and the known composition of the mass would yield great facilities in the calculation of its attraction. Lastly, the deviation of the plummet might be observed on all the four sides. Should such a project take place, it will be best to take the stations at about one-fourth of its altitude above the base, that being the place where the deviation of the plummet would be the greatest. Finally, so favourable for such an experiment do those circumstances appear, and so anxious are my wishes for its completion and success; that, were it not for my great age and little health, I should be glad to make one in any party to undertake so interesting an expedition.

Bedford Row, March 17, 1821. CHARLES HUTTON.

II. *On Light.* By ANDREW URE, *M.D. Professor of the Andersonian Institution, Glasgow.*

[Concluded from vol. lvii. p. 418.]

III. *Polarization of Light.*

This new branch of optical science, sprung from the ingenuity of Malus. It has been since cultivated [illegible] Biot in France, and by Dr. Brewster in this kingdom. [illegible] happy to observe, that Mr. Herschel has lately [illegible] under very favourable auspices.

If a solar ray fall on the [illegible] surface of [illegible] mirror or plate, making an angle with it of [illegible], the [illegible] be reflected in a right line, [illegible] the angle of reflection [illegible] be equal to the angle of incidence. [illegible] point [illegible] path, receive on another plane of [illegible] glass [illegible] in general a second partial reflection. But this reflection will vanish, or become null if the second plate of glass form an angle of 35° 25′ with the first reflected ray, and at the same time be turned, so that the second reflection is made in a plane perpendicular to that in which the first reflection takes place. For the sake of illustration, suppose that the plane of incidence of the ray on the first glass, coincides with the plane of the meridian, and that the reflected ray is vertical. Then, if we make the second inclined plate revolve, it will turn around the reflected ray, forming always with it the same angle; and the plane in which the second reflection takes place, will necessarily be directed towards the different points of the horizon, in different azimuths. This being arranged, the following phænomena will be observed.

When the second plane of reflection is directed in the meridian, and consequently coincides with the first, the intensity of the light reflected by the second glass is at its maximum.

In proportion as the second plane, in its revolution, deviates from its parallelism with the first, the intensity of the reflected light will diminish.

Finally, when the second plane of reflection is placed in the prime vertical, that is east and west, and consequently perpendicular to the first, the intensity of the reflection of light is absolutely null on the two surfaces of the second glass, and the ray is entirely transmitted.

Preserving the second plate at the same inclination to the horizon, if we continue to make it revolve beyond the quadrant now described, the phænomena will be reproduced in the inverse order; that is, the intensity of the light will increase, precisely as it diminished, and it will become equal, at equal distances from the east and west. Hence, when the second plane of reflection returns once more to the meridian, a second maximum of intensity equal to the first recurs.

From these experiments it appears, that the ray reflected by the first glass, is not reflected by the second, under this incidence, when it is presented to it by its east and west sides; but that it is reflected, at least in part, when it is presented to the glass by any two others of its opposite sides. Now, if we regard the ray as an infinitely rapid succession of a series of luminous particles, the faces of the ray are merely the successive faces of these particles. We must hence conclude that these particles possess faces endowed with different physical properties, and that, in the present circumstance, the first reflection has turned towards the same sides of space, similar faces, or faces equally endowed at least with the property under consideration. It is this arrangement of its molecules which Malus named the *polarization* of light; assimilating the effect of the first glass to that of a magnetic bar, which would turn a series of magnetic needles, all in the same direction.

Hitherto we have supposed that the ray, whether incident or reflected, formed with the two mirror plates, an angle of 35° 25′; for it is only under this angle that the phænomenon is complete. Without changing the inclination of the ray to the first plate, it we vary never so little the inclination of the second, the intensity of the reflected light is no longer null in any azimuth, but it becomes the feeblest possible in the prime vertical, in which it was formerly null.

Similar phænomena may be produced by substituting for the mirror glasses, polished plates, formed for the greater part of transparent bodies. The two planes of reflection must always remain rectangular, but they must be presented to the luminous ray, at different angles, according to their nature. Generally, all polished surfaces have the property of thus polarizing, more or less completely, the light which they reflect under certain incidences; but there is for each of them a particular incidence, in which the polarization it impresses is most complete, and for a great many, it amounts to the whole of the reflected light.

When a ray of light has received polarization in a certain direction, by the processes now described, it carries with it this property into space, preserving it without perceptible alteration, when we make it traverse perpendicularly a considerable mass of air, water, or any substance possessed of single refraction. But the substances which exercise double refraction, in general alter the polarization of the ray, and apparently in a sudden manner, and communicate to it a new polarization of the same nature, but in another direction. It is only in certain directions of the principal section, that the ray can escape this disturbing force. The following may be regarded as the most general view of this subject.

When the particles of light pass through a crystallized body, endowed with double refraction, they experience different movements round their centre of gravity, which depend on the nature of the forces which the particles of the crystal exercise on them. Sometimes the effect of these forces is limited to the above *polarization*, or to the arranging all the particles of one ray, parallel to each other, so that their homologous faces are turned towards the same parts of space. When this disposition occurs, the luminous molecules preserve it, in the whole extent of the crystal, and experience no more movement around their centre of gravity. But there exist other cases, in which the molecules that traverse the crystal are not fixed in any constant position. During the time of their passage, they oscillate round their centre of gravity, with velocities, and according to periods, which may be calculated. Lastly, they sometimes revolve round their own axes, with an uninterrupted movement of rotation. The former is called *fixed* polarization, the latter *moveable*.

In the Phil. Trans. for 1813, we have the first of a series of very interesting papers on polarized light by Dr. Brewster. This relates chiefly to some curious properties of agate. The plate of agate which he employed, was bounded by parallel faces, was about the fifteenth of an inch thick, and was cut into a plane, perpendicular to the laminæ of which it was composed. When the image of a taper reflected from water at an angle of 52° 45′, so as to acquire the property discovered by Malus, was viewed through the plate of agate, so as to have its laminæ parallel to the plane of reflection, the flame appeared perfectly distinct; but when the agate was turned round, so that its laminæ became perpendicular to the plane of reflection, the light which formed the image of the taper suffered total reflection, and not one ray of it penetrated the agate. If a ray of light incident upon one plate of agate is received, after transmission, upon another plate of the same substance, having its laminæ parallel to those of the former, the light will find an easy passage through the second plate; but if the second plate has its laminæ perpendicular to those of the first, the light will be wholly reflected, and the luminous object will cease to be visible.

In a second important communication in 1814, on the affections of light transmitted through crystallized bodies, after suggesting that the cultivation of this department of physics may enable us to explain the forms and structure of crystallized bodies, a prediction which he himself has since happily fulfilled, the Doctor states, that if the light polarized by agate, is incident at a particular angle upon any transparent body, so that the plane of reflection is perpendicular to the laminæ of the agate, it will experience a total *refraction;* if it is transmitted through another plate of agate, having its laminæ at right angles to those of the plate by which the light is polarized, it will suffer total *reflection;* and if it is examined by a prism of Iceland crystal, turned round in the hand of the observer, it will vanish and reappear in every quadrant of its circular motion. The pencil of rays to which this remarkable property is communicated, is surrounded by a large mass of nebulous light, which extends about 7° 30′ in length, and 1° 7′ in breadth, on each side of the bright image. This nebulous light never vanished with the bright image which is inclosed, but was obviously affected with its different changes, increasing in magnitude as the bright image diminished, add diminishing as the bright image regained its lustre. Light polarized by the agate, or by any other means, is depolarized, or partly restored to its original state, by being transmitted in a particular direction through a plate of mica, or any other crystallized body.

IV. *Of the Production of Light.*

Some philosophers refer the origin of all luminous phænomena to the sun, whose beams are supposed to penetrate, and combine with, the different forms of terrestrial matter. But we learn from Scripture, that light pre-existed before this luminary, and that its subsequent condensation in his orb, was a particular act of Almighty Power. The phosphorescence of minerals, buried since the origin of things in the bowels of the earth, coincides strictly with the Mosaic account of the creation. We shall therefore regard light, the first-born element of Chaos, as an independent substance, universally distributed through the mineral, vegetable, and animal world, capable of being disengaged from its latent state by various natural and artificial operations. These are

1. Friction.

To this head belong electrical light, and that evolved from the attrition of pieces of quartz, even under water.

2. Condensation and expansion. If atmospheric air or oxygen be suddenly compressed in a glass syringe, or if a glass ball, filled with the latter, be suddenly broke *in vacuo*, a flash of light is instantly perceived.

3. Heat. If air which has been heated up to 900° of Fahren., and which is in itself obscure, be made to fall on pieces of metal, earth, &c. it will speedily communicate to them the power of radiating light. The brilliant flame exhibited in the burning of charcoal and phosphorus, is shown, in the article Combustion, to be merely the ignition of the solid particles of these bodies. At a certain elevation of temperature, about 800° Fahr., all solid bodies begin to give out light. The same effect is produced *in vacuo* by transmitting voltaic electricity through a metallic wire. To this section, we must also refer the phosphorescence of minerals. This curious phænomenon seems to have been first described by Benvenuto Cellini, in his Treatise on Jewellery, published near the beginning of the 16th century. In the year 1663, Mr. Boyle observed, that diamond, when slightly heated, rubbed, or compressed, emitted a light almost equal to that of the glowworm.

The most complete account which we have of mineral phosphorescence, is that recently given by Dr. Brewster in the first volume of the Edinburgh Phil. Journal. His method of examination was ingenious and accurate. He never reduced the body to powder, but placed a fragment of it upon a thick mass of hot iron, or, in delicate experiments, introduced it into the bottom of a pistol barrel, heated a little below redness.

The following Table presents his Results:

Sulphate of barytes gives a bright green light.	
Carbonate,	Do. less brilliant.
Acetate of potash,	Brilliant green light.
Succinic acid,	Do. more durable.
Loaf sugar,	Do.
Selenite,	Do. but transient.
Rock-crystal,	Light red, and then white.
Quartz,	Dull white light.
Borax,	Faint green light.
Boracic acid,	Bright green light.

Mr. Skrimshire has given an extensive catalogue of such substances in Nicholson's Journal, 8vo. vols. 15, 16, and 19. He shows that Canton's pyrophorus yields more light by this treatment than any other body; but that almost every native mineral, except metallic ores and metals, becomes more or less luminous after the electric explosion. A slate from Colly Weston, Northamptonshire, which effervesced with acids, gives a beautiful effect. When the explosion of a jar is taken *above* the centre of a piece some inches square, not only the part above the discharging rods is luminous, but the surface of the plate appears bespangled with very minute brilliant points to some distance from its centre; and when the points of the dischargers rest upon the surface of the slate, these minute spangles are detached, and scattered about the table in a luminous state.

5. Light emitted during chemical changes independent of heat, or in which no perceptible heat is developed. The substances from which such light is emitted, are principally the following:

Marine animals, both in a living state and when deprived of life. As instances of the first may be mentioned the shell-fish called *Pholas*, the *Medusa phosphorea*, and various other *Mollusca*. When deprived of life, marine fishes, in general, seem to abound with this kind of light. The flesh of quadrupeds also evolves light. In the class of insects, are many which emit light very copiously, particularly several species of *Fulgora*, or lantern-fly; and of *Lampyris*, or glow-worm, also the *Scolopendra electrica*, and a species of crab called *Cancer fulgens*. Rotten wood is well known to evolve light copiously, as well as peat-earth.

Dr. Hulme in an elaborate dissertation on this light, published in the Phil. Trans. for 1790, establishes the following important propositions:

1. The quantity of light emitted by dead animal substances, is not in proportion to the degree of putrefaction in them, as is commonly supposed; but, on the contrary, the greater the putrescence, the less light is evolved. It would seem, that this element, endowed with pre-eminent elasticity, is the first to escape from the condensed state of combination in which it had been

imprisone

imprisoned by the powers of life; and is followed, after some time, by the relatively less elastic gases, whose evolution constitutes putrefaction.

2. This light is a constituent chemical principle of some bodies, particularly of marine fishes, from which it may be separated by a peculiar process, retained and rendered permanent for some time. A solution of one part of sulphate of magnesia, in eight of water, is the most convenient menstruum for extracting, retaining and increasing the brilliancy of this light. Sulphate and muriate of soda, also answer in a proper state of dilution with water. When any of the saline solutions is too concentrated, the light disappears, but instantly bursts forth again from absolute darkness, by dilution with water. I have frequently made this curious experiment with the light procured from whiting. Common water, lime-water, fermented liquors, acids even very dilute, alkaline leys, and many other bodies, permanently extinguish this spontaneous light. Boiling water destroys it, but congelation merely suspends its exhibition; for it reappears on liquefaction. A gentle heat increases the vividness of the phænomenon, but lessens its duration.

We shall conclude the subject of Light with the following important practical fact and practical problem.

1. Count Rumford has shown that the quantity of light emitted by a given portion of inflammable matter in combustion, is proportional in some high ratio to the elevation of temperature: and that a lamp having many wicks very near each other, so as mutually to increase their heat, burns with infinitely more brilliancy than the Argand's lamps in common use.

2. To measure the proportional intensities of two or more lights. Place them a few inches asunder, and at the distance of a few feet or yards from a screen of white paper, or a white wall. On holding a small card, near the wall, two shadows will be projected on it, the darker one by the interception of the brighter light, and the lighter shadow by the interception of the duller light. Bring the fainter light nearer to the card, or remove the brighter one further from it, till both shadows acquire the same intensity; which the eye can judge of with great precision, particularly from the conterminous shadows at the angles. Measure now the distances of the two lights from the wall or screen, square them, and you have the ratio of illumination. Thus, if an Argand flame, and a candle, stand at the distances of 10 feet and 4 feet, respectively, when their shadows are equally deep, we have 10^2 and 4^2, or 100 and 16, or 6¼ and 1, for their relative quantities of light.*

III. *Answers by* Dr. WM. BURNEY *to the Queries proposed by* JOHN FAREY, *Esq. Sen., in Phil. Mag. for June, respecting Shooting Stars and Meteors.*

Gosport Observatory, June 11, 1821.

SIR,—IN answer to your 1st Query,—I do not think that so small a portion of light as that produced by reflection from the moon to our atmosphere, when she is only *one* or *two* days old, is "sufficient to obscure numerous of the smallest and medium shooting Stars." The moon at that age is too near her conjunction with the sun, and the light which she then reflects is fainter than that reflected from either Jupiter or Venus when they are on the meridian two or three hours after sunset, at which time these planetary lights are sufficient to produce shadows of objects on the ground; whereas, it does not appear that the moon's light at that age will do so: yet the rays of Jupiter or Venus, from my observations, do not obscure the smallest shooting stars at a distance of 30° or 35° from them. The moon *from her first to her third quarter* may afford enough light to obscure the very smallest and highest of them, but not the middle-sized meteors that are formed low in the atmosphere. Indeed, an observation of Dr. E. D. Clarke's is recorded in the 11th volume of the Annals of Philosophy, pages 273 and 4, of his having been an eye-witness to the perpendicular descent of a brilliant meteor to within 15° of the horizon, at 2 o'clock P.M. on the 6th of February 1818, when it was opposed to the full rays of the sun in a cloudless sky. The Doctor then thought that its appearance was entirely due to the heat and light evolved during the transition of the body from the aerial form to the solid state. It was seen at Swaffham nearly at the same time; also at Norwich and in Lincolnshire. A large meteor of an irregular shape, and perhaps of a similar quality, was also seen in its descent, from a considerable height, apparently to the surface of the sea near St. Helen's in the unobstructed rays of the sun at midday, by a gentleman of my acquaintance, while he was walking along the shore near Haslar hospital in June 1816. I also have registered in my Meteorological Observations, published in The Naval Chronicle, and, since the discontinuance of that work, in Gold's London Magazine, the appearances of many brilliant meteors of the largest sort, while the moon has shone in an unclouded sky and nearly at the full. Hence it appears that the most perpendicular and unobstructed rays, of both the sun and the moon, are not capable of obscuring the largest meteors.

To the 2d Query I reply,—That with a clear sky, shooting stars may be of frequent occurrence at all seasons, and in every portion of visible space. But from tolerably attentive observations on them during the last four years, their number in the summer months, compared with that in the winter, is as 4 to 1. In endeavouring to search for a cause, we naturally attribute this to the additional heat of the air in summer, as afforded by solar influence.

To the 3d Query,—I beg to say that meteors shoot in all directions beneath their visible altitudes; for I have never seen any one of them ascend, except it had met with the resistance of some object on or near the ground; although I have witnessed those of the apparent size of Jupiter come down in the summer evenings nearly to the top of my observatory. If their absolute gravity by the process of condensation be such as not to be obstructed in their motion by the resistance of the medium in which they move, the allusion to their ascension is not consistent with the known laws of gravitation.

In reply to the 4th Query,—I answer that it is possible. Because I have often seen the luminous tails or trains that have been left by some meteors shooting in almost a horizontal direction, full three seconds of time after the disappearance of the ignited bodies from which they had emanated.

To the 5th Query,—I have only to say, that without an experimental proof I should be unwilling to throw out any positive assertion, that the whole appearances of shooting stars and meteors are referrible to one class of bodies; or, if I rightly comprehend this question, that they are all generated by similar atmospherical properties. Among all the ancient and modern conflicting opinions of the cause of igneous meteors, it is to be regretted that we are still left in doubt, from want of experiments, which, unfortunately, appear to be beyond the reach of human ingenuity.

My first object for registering the different sorts of meteors (in connexion with a variety of other atmospherical phænomena) was not purposely to ascertain their cause, but to endeavour to trace whether and what effects they would have on the weather: and I have found that they are generally succeeded by strong gales of wind, &c. but not from any particular point of the compass, as some observers have attempted to prognosticate by their direction.

In regard to their classes, I would recommend to Mr. Farey to read Mr. Forster's "Researches about Atmospheric Phænomena," with which, if he has not already perused that work, he will be much gratified. The meteors or balls of fire, however, that I have sometimes seen descend from thunder clouds to and near the ground during a storm, should (I think) be classed separately from the others; as it is very probable that their embodied forms are generated by a rapid accumulation and condensation of electricity in, and its ultimate dispersion from, the clouds positively charged. The converging and diverging motions of insulated pith-balls suspended by fine flaxen threads from brass wires, and the electric sparks drawn from insulated metallic rods during a thunder-storm, and sometimes during the passage of a *nimbus*, strengthen this opinion. I have often thought that the friction of the peculiar properties of a dry portion of the air, or the gaseous exhalations from the earth (in which we may suppose the absence of electricity), may be the cause of accensions, or the appearance of the small and middle sort of meteors. From these considerations, does it not appear very probable, that the gaseous exhalations from the earth, and also that condensed electricities, in combination with the properties of the atmosphere, have been the natural cause of the largest and most ponderous *Meteorites* that have so often, and in most situations, fallen on the earth's surface, and from great elevations?

To the 6th Query,—I answer that it may be practicable, but fear that, even in the present improving state of science, few men of equal ability and skill in the doctrine of meteors are to be found, compared with those versed in meteorology. For without being possessed of a competent knowledge of the science, and of proper instruments for the purpose, their observations would be rendered vague and erroneous.

The 7th, 8th, and 9th Queries, which require time and new observation to solve, must be left unanswered for the present. However, I beg to say, with all due deference to Mr. Farey, as a former observer of atmospherical phænomena, that I cannot for a moment agree with his opinion, that the chain of facts relating to the greatest and most conspicuous meteors, is sufficient for reckoning all these bodies to the class of *Satellitulæ* of the Earth. This seems a new and bold idea, probably suggested to him from perusing the Chronological History of *Meteorites* in the 14th volume of the Edinburgh Encyclopædia; as in that article it is stated that similar meteoric appearances have formerly been seen on different nights. In all my observations on the largest sort of meteors, I have never seen any striking coincidence in their appearances and motions, that could rationally suggest the idea of their being *Satellitulæ* of the Earth.

IV. *Remarks on the Gradation of Heat in the Atmosphere.*

By James Ivory, *M.A. F.R.S.*

The communication of heat between the earth and the atmosphere depends on so many circumstances, and follows laws so extremely complicated, that very little is exactly known on the subject. The alternate change of day and night; the winds produced by the unequal action of the sun's rays in different regions;

the greater or less degrees of moisture that prevail, are some of the causes that have most influence, more particularly on the temperature of the stratum of air in the immediate vicinity of the earth. But there is a predominant cause, which makes the temperature continually diminish as we ascend to greater heights in the atmosphere. We allude to that property of air, by which it absorbs heat when it expands by being less compressed. A portion of air that has become heated at the earth's surface, rises upward on account of its diminished density; as it ascends, the pressure being less, it expands and becomes colder by absorbing heat; and hence the velocity of its ascent decreases, and it finally comes to rest when its density is reduced to that of the surrounding mass. The property which air possesses of becoming colder by rarefaction, checks the elastic force by which it tends to fly off from the earth. It operates along with gravity to make that fluid cling to the earth, and to impose a limited boundary on the atmosphere.

All our knowledge of the gradation of heat in the atmosphere, is derived from observations of the barometer and thermometer made for the purpose of measuring heights. The exactness of such measurements, within certain limits, cannot be questioned; because they have been verified in so many instances by comparing the like results obtained by levelling and by the operations of geometry. We may therefore presume, that the principles on which is founded the rule for calculating heights by the barometer, are nearly correct. Hence the great improvement of this method, introduced by De Luc, of estimating the temperature of the column of air at a mean between the temperatures observed at its extremities, must be at least a near approach to the truth. This estimation is equivalent to supposing that the heat decreases uniformly from the bottom to the top of the column; which law, if it be not rigorously exact, so covers the actual variations that they become insensible to observation.

Admitting that the heat in a column of air diminishes in the same proportion that the height increases; if we knew the rate of decrease, or the elevation necessary for depressing the thermometer one degree, we should be able to deduce the temperature at any altitude in the atmosphere from the temperature at the earth's surface; and likewise to compute the height of a column of air, from having given the temperatures at its extremities. To find the rate, we must have recourse to experiments. But a very slight attempt to determine this quantity is sufficient to show that it is extremely variable, even in circumstances in which it is impossible to discern any apparent difference. In 38 measurements recorded by Ramond, made in circumstances very various, from observations free from the suspicion of great errors and leading to results sufficiently exact, the quickest decrease of heat is at the rate of 61 fathoms, and the slowest, at the rate of 136 fathoms, to a centesimal degree. The mean rate deduced from all the 38 observations is almost exactly 90 fathoms; and we see that the extremes are different from the mean quantity, not by a small part of it, but by a half. Now, as we cannot doubt that the principle which distributes the difference of temperatures equally through the whole height of the column is nearly true, we must infer that the rate of decrease depends in a great measure upon circumstances peculiar to each particular case. In this respect causes seem to operate, which the observer is not only unable to appreciate, but even of the existence of which he has no indications. Very little confidence can therefore be placed in temperatures at different heights in the atmosphere, estimated by the rate of the decrease of heat; although the exactness of barometrical measurements is not by this means affected, the heat of the column of air being always determined by the temperatures actually observed at its extremities.

Perhaps we may find, in the nature of the instrument with which the heat is measured, some reason for the irregular deviations of the observed temperatures of the atmosphere from any theoretical law. The thermometer measures the temperatures only of such bodies as are in immediate contact with it. Local circumstances, impossible to be appreciated, may therefore so much affect a thermometer placed at the extremity of a column of air, as to make it indicate a temperature very different from what would take place at a medium, and when all the causes that influence the propagation of heat through its whole length, have produced their due effect. In this manner the observed may diverge from the true temperature by a current of air in which the thermometer is placed; by the reflection of the sun's rays from the neighbouring objects; by evaporation and the radiation of heat depending upon the nature of the soil in the vicinity, and by other causes.

The rate of the decrease of heat deduced from the above-mentioned observations of Ramond; that is, 90 fathoms to a centesimal degree, or 100 yards to one degree of Fahrenheit; seems to be the quantity most generally adopted. By means of this proportion, the temperatures that prevail at given altitudes in the atmosphere are sometimes determined with great precision, although in other cases the calculation is wide of the truth. Thus, taking the great height of 3817 fathoms ascended by Gay-Lussac in a balloon, the difference of temperature, at the rate of 90 fathoms to a degree, will be found 42°·4, which is a near approximation to 40°·3, the observed quantity. On the other hand, if we apply the same rule to the extreme cases in the Table of Ramond,

mond, we shall obtain results quite erroneous and unsatisfactory. The decrease of heat in the atmosphere, as determined by the ascent of balloons, seems to follow a slower rate than in the case of altitudes on the earth's surface. There can be no doubt that this manner of experimenting is free from many causes of irregularity to which terrestrial observations are subject.[*] We might therefore hope that by this means much light would be thrown on the gradation of heat in the atmosphere; but a sufficient number of accurate experiments are wanting to establish a conclusion in which confidence can be placed. In the case of the ascent of Gay-Lussac, we obtain a rate of nearly 95 fathoms to a centesimal degree, which is not extremely different from the mean found by terrestrial altitudes.

If we could abstract from the many and powerful causes by which the natural and regular propagation of heat in the atmosphere is continually disturbed, there is no doubt that the temperature would decrease nearly in the same proportion that the height increases. But this must not be understood in a sense strictly literal and mathematical. If we conceive the height of a column of air to be divided into portions corresponding to the same given difference of temperature, it is much more probable that these portions will form a progression increasing or decreasing slowly, than that they will constitute a series of perfectly equal increments. Such however are the anomalies attending observations of the temperature of the atmosphere, that it is extremely difficult to determine by experiment, whether the heat decreases in a less or greater ratio than the height increases. Accordingly the opinions of philosophers on this point are divided. The late Professor Playfair, in his Outlines of Natural Philosophy, supposes that the increment of altitude necessary for depressing the thermometer one degree, is a quantity continually increasing as we ascend higher. But the contrary opinion, that the heat decreases more rapidly than the height increases, is more generally prevalent; and it seems to deserve the preference, because it is adopted by those philosophers, such as Humboldt and Ramond, who have more particularly directed their attention to this research.

We may draw from the theory of the astronomical refractions an argument in favour of the conclusion, that the heat of the atmosphere decreases in a greater ratio than the height increases. It has hitherto been found to be impossible to reconcile the horizontal refraction of the stars with the actual rate of the decrease of heat in the atmosphere. If we adopt the law of a uniform decrease of temperature, and take the rate at 90 fathoms to a centesimal degree as found by experiment, the horizontal refraction thence determined will exceed the true quantity by about a minute of a degree. In order to reconcile the theory with astronomical observations, the elevation for one degree of depression of the thermometer must be diminished by a fifth part, or rather more. If, instead of a uniform decrease of temperature, we suppose that the increments of altitude for one degree of difference of temperature form an increasing progression, the error of the horizontal refraction will be greater than before; and, in order to correct it, the initial rate of the decrease of heat must be made still more rapid than in the former supposition. On the other hand, if we adopt the opposite law, and suppose that the increments of altitude for one degree of the thermometer, form a series of decreasing quantities, the horizontal refraction will be less than it would be if the initial rate of the decrease of heat continued progressively uniform. Thus, while the two first laws are contrary to experience, it may be possible, by making the variation of temperature according to the last supposition sufficiently rapid, so to correct the excess of refraction arising from the actual decrease of heat at the earth's surface, as entirely to reconcile the theory with observation. We are indeed in possession of no solution of the problem of the atmospherical refractions that proceeds upon the supposition mentioned; but, in the present state of our knowledge, the argument is not less conclusive in favour of the law, that the heat of the atmosphere decreases in a greater ratio than the height increases.

Professor Leslie, of Edinburgh, has given a precise and mathematical theory of the variation of heat in the atmosphere. If h denote the height of the mercury in a barometer at the lower of two stations, and β the like height at the upper one; then, t being the difference of temperature in centesimal degrees, we have the relation between the quantities, viz.

$$t = 25\left(\frac{h}{\beta} - \frac{\beta}{h}\right).$$

This formula was first published in 1811, in the notes to the second edition of the author's Geometry, p. 495. In the article CLIMATE in the Supplement to the *Encyclopædia Britannica*, it appears in a form somewhat different, the ratio of the densities at the extremities of the elevation, being substituted for that of the barometrical pressures. Strictly speaking, the two ratios are not equivalent; because at the top of the column the temperature is always less than it is at the bottom, and the density of a mass of air depends both on the pressure and the temperature; but, as in all the examples adduced in the article CLIMATE, the density is estimated by the pressure alone, it seems to have been the author's intention to make no distinction between the two formulæ.

ly we may esteem the ingenuity and sagacity in the experimental investigation of the formula, it [illegible] hardly possible to ascertain, from that process alone, the degree of confidence that ought to be placed in its accuracy. The circumstances attending the experiments are such as to make it more wonderful that the author has been able to deduce from them a result at all conformable to nature, than one of a doubtful character only, and requiring to be confirmed by comparison with actual observation. The decision to which such a comparison has led seems to be this; that the formula is pretty accurate for small elevations, but that, in the case of greater, it determines the difference of temperature considerably above the truth. It agrees with observation at the earth's surface; but, as we ascend in the atmosphere, it makes the increments of altitude corresponding to a given difference of temperature decrease too swiftly.

On account of the great simplicity of the formula, it would be very useful in many researches, if its claim to accuracy were established in a satisfactory manner. It would, for instance, supply a desideratum in the problem of the atmospherical refractions, for which purpose indeed it has already been applied. Having entertained the idea of comparing it with the common method of calculating heights by the barometer, I find that by this means a criterion may be obtained that will enable us to form a correct opinion on the point in question.

Using the same letters as before to denote the barometrical pressures at the bottom and top of a column of air, the height of which in fathoms is equal to x; and neglecting the correction depending on temperature as unnecessary in the present inquiry, we get by the usual rule,

$$10000 \times \log. \frac{h}{\beta} = x^{*}.$$

In this formula the logarithms are of the common sort; and, as the ratio of the common to the hyperbolic logarithms is that of 4343 to 10000, we have $10000 \times \log. \frac{h}{\beta} = \cdot 4343 \times \text{hyp. log. } \frac{h}{\beta}$; wherefore,

$$\text{Hyp. log. } \frac{h}{\beta} =$$

For the sake of abridging, put $a = \frac{1}{4343}$; then, c being the base of the hyperbolic logarithms, we readily get these equations, viz.

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[*] Professor Playfair's Outlines, § 341, p 217, vol. i.

Hyp.

$$\text{Hyp. log. } [illegible] \; ax,$$

[illegible]

$$\frac{\beta}{b} = [illegible]$$

[illegible] expanding in a series,

$$\frac{b}{\beta} - \frac{\beta}{b} = 2ax \times \{1 + [illegible] + \&c.$$

If we allow 90 fathoms of altitude to every centesimal degree of decrease of temperature, which is the rate adopted by Professor Leslie himself, we have $x = 90 \times t$; wherefore,

$$\frac{b}{\beta} - \frac{\beta}{b} = 180.at \times \{1 + \frac{1}{6} \times (90.at)^2 + \&c.$$

But $180a = [illegible] = \cdot 0411 = \frac{1}{25}$ nearly; and $90a = \cdot 0207 = \frac{2}{100}$ nearly; and hence,

$$25\left(\frac{b}{\beta} - \frac{\beta}{b}\right) = t \times \{1 + \frac{2}{3} \times \left(\frac{t}{100}\right)^2 + \&c.$$

Now, for small differences of temperature, the series on the right-hand side may be considered as equal to unit, and then we get

$$25\left(\frac{b}{\beta} - \frac{\beta}{b}\right) = t,$$

which is no other than Professor Leslie's formula. The accuracy of the Professor's theory is therefore confirmed for moderate elevations; but then it is proved to be equivalent in such cases to the more simple law of a uniform decrease of heat, the only difference being, that the barometrical pressures are used instead of the altitudes to which they belong. In the case of great altitudes, and considerable differences of temperature, the series will be no longer equal to unit, and the formula will diverge from the theory of barometrical measurements. Thus in Gay-Lussac's ascent, t being about 40°, the difference of the two methods will be about 4° or 5°, or a tenth of the whole, the Professor's formula being farther from the truth. In reality the difference will be greater, because it is augmented by the correction for temperature in the barometrical formula, which has been neglected in the foregoing investigation. It would be easy to supply this defect, but it appears hardly necessary. It has been sufficiently proved, that for moderate elevations Professor Leslie's formula is equivalent to the law of a uniform decrease of heat, and that in great altitudes it is equally at variance with observation and the theory of barometrical measurements.

It is extremely probable that, in regard to the astronomical refractions, Professor Leslie's formula will be found to be equivalent to the law of a uniform gradation of temperature in the atmosphere, the rate at which the heat decreases depending upon the constant coefficient. If this conjecture shall turn out to be just, it must be allowed that the introducing of the formula can have no other effect than to lead off the attention from the true principles and the real difficulties of the problem.

July 5, 1821 J. IVORY.

N. B. The readers of the Philosophical Magazine are desired to read *sin. A* for *sin.* in the formula at p. 406 of the last Number.

V. *On the black Rete mucosum of the Negro being a Defence against the scorching Effect of the Sun's Rays. By Sir* EVERARD HOME, *Bart. F.R.S.**

To ascertain the use of the black colour of the rete mucosum in the Negro, has occupied the attention of many physiologists; and I confess that this subject formed the first investigation in which I ever engaged. Fruitless, indeed, were my attempts; and when I learnt that black surfaces absorbed heat, and raised the temperature several degrees beyond any others, I gave the matter up in despair. Two years ago my attention was again called to this inquiry, upon being told by our late excellent President, that a silver fish, in a pond at Spring Grove, during a very hot summer, immediately after some trees by which the pond was shaded were cut down, was so much exposed to the sun's rays as to have its back scorched, the surface putting on the same appearance as after a burn, and rising above the scales of the surrounding skin. I saw the fish several times, and directions were given to send it to me when it died; but I was not so fortunate as to receive it.

This extraordinary circumstance brought to my recollection one not less so. In crossing the Tropic in April 1781, at twelve o'clock at noon, in a voyage to the West Indies, I had fallen asleep upon deck, lying upon my back, having a thin linen pair of trowsers on, and I had not slept half an hour, when I was awakened by the bustle attending the demand of forfeits on crossing the Line, and found the inside of the upper part of both thighs scorched, the effects of which have never gone off: but till now I could not imagine how it happened, always suspecting it to be the effect of the bites of insects; but I never satisfied myself upon that subject.

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* From the Transactions of the Royal Society for 1821. Part I.

The effect of the sun's rays upon the fish under water, led me to suspect the mixture of light and heat to be the cause of this scorching effect

To ascertain the truth of this opinion, I made the following experiments.

Exp. 1.—In August 1820, I exposed the back of my hand to the sun at twelve o'clock, with a thermometer attached to it, another thermometer being placed upon a table, with the same exposure. That on my hand stood at 90°, the other at 102°. In 45 minutes blisters rose, and coagulable lymph was exuded, which became vascular under my eye: the pain was very severe.

Exp. 2.—I exposed my face, my eyelids, and the back of my hand to water heated to 120°: in a few minutes they became painful; and when the heat was farther increased, I could not bear it.

Exp. 3.—I exposed the backs of my two hands to the sun's rays, with a thermometer upon each; the one hand was uncovered, the other had a covering of black cloth, under which the ball of the thermometer was placed. After ten minutes, the degree of heat of each thermometer was marked, and the appearance of the skin examined. This was repeated at three different times. The

1st time the thermometer under the cloth 91°, the other 85°
2d time 94 91
3d time 106 98

In every one of these trials the skin was scorched that was uncovered; the other had not suffered in the slightest degree; there was no appearance of perspiration on either hand.

Exp. 4.—The back of a Negro's hand was exposed to the sun with a thermometer upon it, which stood at 100°; at the end of ten minutes the skin had not suffered in the least.

Exp. 5.—During the eclipse of the sun on September 7, 1820, I exposed the back of my hand to the rays concentrated by a double lens of half an inch focus, at three different periods of the eclipse. When the heat to a thermometer was 75°, that is from 47 to 57 minutes past one o'clock, the concentrated rays felt warm, but gave no pain, although applied for ten minutes.

When the heat to a thermometer was 79°, that is at 15 minutes past two o'clock, the concentrated rays in four minutes gave pain; in five minutes blistered the skin, and produced dots of coagulable lymph, which became vascular under the eye.

When the heat to a thermometer was 82°, that is at half past two o'clock, the concentrated rays in three minutes gave pain; in four, the part was blistered, and the pain could not longer be endured.

Exp. 6.—September 8, 1820, at eleven o'clock, the heat in the sun 90°; the concentrated rays applied to my naked arm produced a vesicle. This experiment was repeated when the heat was 81°, and in seven minutes a blister formed on the arm.

Exp. 7.—September 9th, eleven o'clock, the thermometer in the sun at 90°. The concentrated rays applied to a piece of black kerseymere cloth, made tight round my arm for 15 minutes, gave no real pain, and left no impression whatever on the skin, although the nap of the cloth had been destroyed.

This experiment was repeated with white kerseymere, the heat at 86°; in 15 minutes a blister was formed.

Repeated with Irish linen, the thermometer 86°. In 15 minutes a blister was formed, and coagulable lymph thrown out, which had become vascular.

The same experiment was made with a white handkerchief loose upon the hand, the heat 83°. In 15 minutes an inflammatory blush was produced over a surface of several inches extent, which almost immediately disappeared on withdrawing the hand from the sun's rays.

Exp 8.—September 12th. The sun's heat at noon 85°. The concentrated rays applied to the back of the hand of a Negro from Grenada, for 15 minutes, produced no visible effect; at the first moment he felt a stab going inwards, but that went off, and afterwards he had no pain.

From these experiments, it is evident that the power of the sun's rays to scorch the skin of animals is destroyed when applied to a black surface, although the absolute heat, in consequence of the absorption of the rays, is greater.

The same wise providence which has given so extraordinary a provision to the Negro for the defence of his skin, while living within the tropics, has extended it to the bottom of the eye, which otherwise would suffer in a greater or less degree when exposed to strong light; the retina, from its transparency, allowing it to pass through without injury.

That the nigrum pigmentum is not necessary for vision, but only provided as a defence against strong light, is proved by its being darker in the Negro than the European, and being of a lighter colour in fair people than in dark, and therefore lightest in those countries furthest removed from the effects of the sun.

In the monkey it is dark, and in all animals that look upwards.

In all birds exposed to the sun's rays the nigrum pigmentum is black. In fishes, the basking shark, which lies upon the surface of the ocean, has a nigrum pigmentum. The turbot and skate, which lie upon banks of sand in shallow water, have nigrum pigmentum.

In all ruminating animals and birds of prey, there is a lucid tapetum at the bottom of the eye.

The owl, that never sees the sun, has no nigrum pigmentum.

The mackarel has the bottom of the eye lucid as quicksilver.

The *coup de soleil*, met with in the West Indies, the effects of which I have seen, I attribute to the scorching effect of the sun's rays upon the scalp.

The Egyptian ophthalmia I consider to be the effect of the sun's rays, and the glare of reflected light.

I have stated the fact of the scorching power of the sun's rays being destroyed when they are applied to black surfaces, but have not gone further. Sir Humphry Davy, to whom I showed these observations, immediately explained it. He said the radiant heat in the sun's rays was absorbed by the black surface, and converted into sensible heat.

VI. *On the annular Eclipse of the Sun which will happen on the 15th of May 1836; being the principal Results of Calculations for Greenwich and Edinburgh. By Mr.* GEORGE INNES.

Aberdeen, May 15, 1821.

SIR, — THE great solar eclipse of April 1, 1764, after four *Chaldean* periods, will return again on the 15th of May 1836. It will then be very great to all Europe, and in Great Britain it will be more interesting than any that has happened since 1793, as also than any that will happen before it. Like the eclipse of 1793, it will be annular in Scotland, but not at Greenwich.

The calculation of the general eclipse, the track of the central path of the annulus, its boundaries and extent, I shall reserve for a future communication. In the mean time, I send you the elements for projection and calculation, as also the results of the principal steps of calculations for Greenwich and Edinburgh.

I once intended to send you the calculations at large, but after collecting the whole process into a quarto manuscript, I find that it could not be conveniently printed in octavo.

In making a projection of this eclipse for any particular place, it will be found that the times and digits eclipsed may be determined with almost as great accuracy as the method of projection admits of; the path of the moon, and those of the parallels of latitude, being nearly parallel to one another about the time of greatest obscuration; whereas in that of September last, they formed nearly the greatest angle possible.

The following elements are obtained from the Solar Tables of Delambre, and the Lunar Tables both of Burckhardt and Burg. In calculating for Greenwich and Edinburgh, I have used the Tables of Burckhardt.

The Elements as obtained by using Delambre's Tables along with those of Burckhardt.		h	′	″		But by using those of Burg. h	′	″	
Mean time of ecliptic conjunction at Greenwich, May 15th, at		2	6	44,40	P.M.	2	6	59·98	P.M.
Equation of *mean* to *apparent* time, +3′ 56″,77. Hence the *apparent* time is		2	10	41,17		2	10	56,75	
Horary decrease of the equation of time				,028					
Longitude of the Sun and Moon, each from the true equinox		54·	42	19,61		54°	42	20,69	
Sun's right ascension		52	20	33,66		52	20	34,48	
—— declination north		18	57	47,95					
—— horizontal parallax				8,70					
—— horary motion in longitude			2	24,15					
—— ———— in right ascension, ...			2	28,12					
—— ———— in declination, increasing, ...				35,13					
—— semidiameter, ...			15	50,31					
Obliquity of the ecliptic, ...		23	27	44,14					
Moon's equatorial horizontal parallax, ...			54	24,03			54	25,05	
—— latitude north, increasing, ...			25	11.49			25	47,77	
—— horary motion in longitude at the instant of conjunction, ..			30	0,01			29	59,98	
—— ———— for the hour preceding, ...			30	0,36			30	0,33	
—— ———— for the hour following, ...			29	59,65			29	59,63	
—— horary motion in latitude at the instant of conjunction, ...	+		2	46,48			2	46,05	
—— ———— for the hour preceding, ...	+		2	46,55			2	46,11	
—— ———— for the hour following, ...	+		2	46,42			2	45,99	
—— horizontal semidiameter, ...			14	49,52			14	51,24	
Angle of the relative orbit with the ecliptic, ...		5	44	32,1					
Horary motion of the Moon from the Sun in the relative orbit, ..			27	43,9					

From the preceding Elements, as calculated from the Tables of Delambre and Burckhardt, the following *data* are obtained

	Apparent time	☽'s hor mot in long.	☽'s ½ hor mot in long	☽'s longitude.
	h ′ ″	′ ″	′ ″	° ′ ″
	0 10 41,226	30 5,61	15 2,805	53 42 14,34
	0 40 41,212	30 3,16	15 1,58	53 57 17,145
	1 10 41,198	30 1,41	15 0,705	54 12 18,725
	1 40 41,184	30 0,36	15 0,18	54 27 19,45
Conj.	2 10 41,17	30 0,01	15 0,005	54 42 19,61
	2 40 41,156	29 59,66	14 59,83	54 57 19,44
	3 10 41,142	29 58,61	14 59,305	55 12 18,745
	3 40 41,128	29 56,86	14 58,43	55 27 17,175
	4 10 41,114	29 54,41	14 57,205	55 42 14,38
	4 40 41,1	29 51,26	14 55,63	55 57 10,01
	5 10 41,086	29 47,41	14 53,705	56 12 3,715

	Apparent time	☽'s hor mot. in lat	☽'s ½ hor. mot. in lat	☽'s latitude.
	h ′ ″	′ ″	′ ″	′ ″
	0 10 41,226	2 47,44	1 23,72	20 7,63
	0 10 41,212	2 47,02	1 23,51	21 31,35
	1 10 41,198	2 46,72	1 23,36	22 54,86
	1 40 41,184	2 46,54	1 23,27	24 18,22
Conj.	2 10 41,17	2 46,48	1 23,24	25 41,49
	2 40 41,156	2 46,42	1 23,21	27 4,7
	3 10 41,142	2 46,24	1 23,12	28 27,82
	3 40 41,128	2 45,94	1 22,97	29 50,79
	4 10 41,114	2 45,52	1 22,76	31 13,55
	4 40 41,1	2 44,98	1 22,49	32 36,04
	5 10 41,086	2 44,32	1 22,16	33 58,2

	Apparent time.	Sun's longitude.	Sun's Æ
	h ′ ″	° ′ ″	° ′ ″
	0 10 41,226	54 37 30,71	52 15 37,42
	0 40 41,212	54 38 42,935	52 16 51,48
	1 10 41,198	54 39 55,16	52 18 5,54
	1 40 41,184	54 41 7,385	52 19 19,6
Conj.	2 10 41,17	54 42 19,61	52 20 33,66
	2 40 41,156	54 43 31,835	52 21 47,72
	3 10 41,142	54 44 44,06	52 23 1,78
	3 40 41,128	54 45 56,285	52 24 15,84
	4 10 41,114	54 47 8,51	52 25 29,9
	4 40 41,1	54 48 20,735	52 26 43,96
	5 10 41,086	54 49 32,96	52 27 58,02

Sun's half hourly motion in longitude, .. 1′ 12″,225

in Æ. 1 14,06

In procuring the preceding table of general *data*, I have used the method given by Professor Vince in his Astronomy, vol. iii. p. 52, for finding the horary motions in longitude and latitude, before and after the time for which the places are calculated. Mr. Vince remarks that this method is not perfectly correct, but sufficiently so for the longest eclipse. I am inclined to think it far from probable, that in any case, even when the moon is in her perigee, the half hourly change of her motion can vary in four hours in the ratio of 3, 5, 7, 9. In the example given by Mr. Vince, the equation of the second order in longitude is 1″·24; from which, applying the arithmetical progression, we find that the horary motion at two hours preceding, is increased by 11″·16, and, at two hours following, diminished by the same quantity. Now, suppose the instant for which the places are calculated from the tables, to be an hour before the moon is in perigee; the horary motion will in this case increase for the first hour which follows, and then, having arrived at its maximum, it will begin to decrease. In such a case, therefore, I am at a loss to see how the above method could hold good.

I have been induced to solicit the attention of your valuable correspondents to this point, from a hope that some of them will have the goodness to give, through the medium of your Magazine, some easy, and at the same time more accurate method of applying the equation of the second order, both in longitude and latitude.

The following table exhibits the results of the principal steps of the calculations for Greenwich and Edinburgh. The times marked on the tops of the columns are the instants assumed.

The line marked ☉ contains the Sun's longitude.
———— A ——— the Sun's right ascension.
———— ☽ ——— the Moon's true longitude.
———— L ——— the Moon's true latitude.
———— R ——— the right ascension of the meridian.
———— H ——— the altitude of the nonagesimal.
———— N ——— the longitude of the nonagesimal.
———— P ——— the parallax in longitude.
———— p ——— the parallax in latitude.
———— D ——— the appar. diff. of long. of the ☉ and ☽.
———— a ——— the Moon's apparent latitude.
———— S ——— the Moon's apparent semidiameter.
———— m ——— the Moon's apparent motion in 60″ of time.
———— e ——— the errors from the instants assumed, where — shows the inst. too early, and + too late.

The

The Calculation for Greenwich.

	For the beginning.		For the apparent conjunction.		For the end	
	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″
	1 53 41,178	1 54 41,175	3 22 41,136	3 23 41 136	4 43 41,097	4 44 41,097
⊙	54 41 38 082	54 41 41,09	54° 45 12,95	54 45 15,357	54 48 27,057	54 48 30,365
A	52 19 31,063	52 19 31,161	52 23 31,404	52 23 33,871	52 26 51,306	52 26 53,535
☽	54 33 49,404	54 34 10,47	55 18 18,327	55 18 48,292	55 58 33,544	55 59 9,402
L	24 54,209	24 57,075	29 1,044	29 3,813	32 44,283	32 47,031
R	80 45 9,4	81 0 11,8	103 3 48,4	103 18 50,9	123 22 7,8	123 37 10,5
H	62 1 1,1	62 1 41,5	61 48 30,3	61 47 32,0	59 31 55,1	59 20 20,0
N	83 27 27,5	83 37 13,0	99 14 24,1	99 25 1,9	113 33 2,0	113 43 36,7
P	—23 22,41	—23 29,42	—33 25,51	—33 31,31	—39 39 32	39 42 67
p	27 23,76	25 23,16	25 31,76	25 32,56	27 24 03	27 25 69
D	31 11,03	30 50,06	20,13	1 62	30 32,20	30 56 35
a	29 40 S	26,08 S	3 29,28 N.	5 31,25 N.	5 20,25 N.	5 21,04 N.
S	15 0,43		14 58,43		14 55,01	
m	20,71		21,75		23,87	
e	—21,12	—0,41	—20,13	+1,62	+13,82	+37,69
	Hence, the Eclipse begins at 1h. 54′ 42″,37		Apparent Conj at 3h. 23′ 36″,67.		End of the Eclipse at 4h. 43′ 6″,36.	

The Calculation for Edinburgh; observed Lat. 55° 56′ 42″, and Long. in Time 12′ 49″ W. by requisite Tables.

	For the beginning.		For the apparent conjunction, &c using 3 Instants			For the end	
	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″
	1 35 52,170	1 36 52,170	3 1 52.138	3 2 52,139	3 3 52 139	4 22 52 102	5 23 52,102
☉	54 41 29,545	54 41 20 052	54 44 53,09	54° 44 55,907	54 44 58 505	54° 48 8,097	54 48 11,105
A	52 19 50,840	52 20 41 818	52 23 11,055	52 23 14,123	52 23 16,502	52 20 31,02	52 20 34 085
☽	54 31 10 402	54 31 40,466	55 14 18,637	55 14 48.605	55 15 18,573	55 54 40,057	55 55 10 821
L	24 40,423	24 43,105	25 38,9	28 41,05	28 44,42	32 22,333	32 25 085
R	76 17 42,0	76 32 44.5	97 51 13.7	98 6 16,2	98 21 15,7	118 9 33,2	118 24 35,0
H	57 16 47 9	57 19 40,4	57 30 15,6	57 35 42,3	57 35 0,4	55 55 48,3	55 53 52,4
N	80 52 33,5	81 2 25,0	95 13 40 4	95 23 50 3	95 33 40,5	108 43 10,0	109 53 4.0
P	−20 27,13	−20 34,37	−29 39 32	−20 44.95	−20 50,02	−35 58,05	−36 2,28
p	20 15,05	20 15,17	28 59 04	28 59.40	28 59,62	30 17,19	30 19,09
D	30 34 32	30 13,05	14,57	7 55	20,45	30 33 61	30 57,43
a	4 35,53 S.	4 31,97 S.	20,14 S	17,6 S.	15 5 S.	2 5,14N.	2 0,4 N.
s	15 0.17		14 58,50			14 55,06	
m	20,07		21.92		21.9	23,85	
c	−4 12	+13 25	−14,37	+7.55	+20,45	−8 4	+15.45

Hence, the Eclipse begins at 1h. 36′ 5″,01	Apparent Conjunction at 3h. 2′ 51″,47	End of the Eclipse at 4h 23′ 13″,23.

The other results may now be readily obtained, and in one view they are as follows, *apparent time:*

At Greenwich

The eclipse begins at	1h 51′ 42″,37 P.M.
Greatest obscuration, ...	3 22 41,15
Apparent conjunction, ...	3 23 36,67
End of the eclipse, ...	4 43 6,36
Digits eclipsed at greatest obscuration, on the north part of the sun's disc.	10° 28 58,45

The moon will make the first impression on the sun's disc at 131° 39′ from his vertex on the right hand.

At Edinburgh

The eclipse begins at	1h 45′ 5″,0 P.M.
Beginning of the annulus, ...	3 0 1[illegible],55
Apparent conjunction, ...	3 2 31,17
End of the annulus, ...	3 4 43,72
End of the eclipse, ...	4 23 15,[illegible]
Digits eclipsed during the annulus, ..	11° 33 20,7[illegible]
Breadth of the annulus on the sun's north limb,	1 10,55
- ----- - ———— — on the sun's south limb,	33,14
Proportion of the sun's disc obscured during the annulus	,89105

The moon will make the first impression on the sun's disc at 132° 22½ from his vertex on the right hand.

As I am anxious to determine the error of the Astronomical Tables by means of the observations made on the late solar eclipse, I should be much obliged to you, or Dr. Burney, to communicate, through your Magazine, the latitude and longitude of Dr. Burney's Observatory at Gosport.

I am, dear sir, yours respectfully,

To Dr. Tilloch. Geo. Innes.

VII. *Remarks on Mr.* Riddle's *Claim to the Invention of a new Method of determining the Latitude.*

Edinburgh, June 8, 1821

Sir,—As an article by Mr. Edward Riddle has been inserted in the Philosophical Magazine for May 1821, which appears to me to contain insinuations totally unfounded, I beg leave to say a few words in reply.

General Brisbane having been abroad for some time expresses himself as follows, in his Memoir on the Repeating Circle, Edinburgh Philosophical Transactions, vol. ix. Part I. page 97, &c. and read in February 1819.

"Having had frequent opportunities, during my residence in France, of seeing a great many repeating reflecting circles, several of which I observed with, and having found much consistency in the results deduced from observations made with them; it occurred to me, that if I could engage Mr. Troughton to execute one for me, it would be a most perfect instrument." This was done, it appears from the General's memoir, previous to the 8th of November 1817, and since, the latitude deduced is about 50° 2[illegible]. We believe the observations had been made in France. The General pays a just tribute to Mr. Troughton as an artist, and then adds, "I beg to be permitted to be a little more particular than I otherwise should, as to the manner I have pursued in calculating and deducing the latitudes from the instrument, as it is not by any means *as yet generally known in England*, although I have no doubt that when quite understood it will be found to surpass all other instruments of its size, for simplicity and accuracy; and I am desirous that amateurs may profit from the experience I have had *for some years* of its utility."

"In the communication which," says he, page 101, "I had the honour to submit to the Royal Society of Edinburgh, on the subject of ascertaining time with accuracy, and which was read on the 2d of February 1818," [and it may be added, printed in , and which Mr. Riddle acknowledges having seen in his communication, page 364, vol. lvii. Phil. Mag.,] "I intimated my intention, if that was deemed worthy of a place in the Transactions, to transmit a memoir on the repeating circle, which I now beg leave to lay before them," &c.

Here then it appears that General Brisbane had been in the habit of using this method, and had also published it before Mr. Riddle had said any thing about it, and the memoirs which followed were merely a continuation of the same, or a somewhat similar method. In the mean time Mr. Riddle says he thought it probable (and we are bound to believe him) that his method was the very same as that intimated by General Brisbane, and lays claim to the invention as a new method of determining the latitude. "The method of General Brisbane is more like mine," says Mr. R. "than I was likely to anticipate, as it is absolutely the same both in principle and in all its practical details." Indeed Mr. R. has already told us, page 364, as soon as General Brisbane's method for ascertaining time came to his knowledge, that their methods were the same; and I must take Mr. R.'s word of honour that he practised that method before, or at the time General B. published his method; otherwise I should likely have insinuated that he (Mr. R.) did not practise this method till General Brisbane made it public, which seems to have induced Mr. R. to publish this method of ascertaining the latitude immediately afterwards. But this would *reverse* the claims! Mr. R. finds that the theorems of correction are precisely the same. We beg leave to remark, that there is a typographical error in the denominator of the first factor of the first term in General B.'s formula, which he tells us with an honest frankness he got from Delambre. This however is corrected in the second line below, though one still remains in the second term in both places. As Mr. R. says the two are the same, his of course partakes of the same error, which we wonder much he has not corrected! "But," says Mr. R. on the subject of the *theorem*, "I dare say neither of us fancies he has made any discovery." I do not know what Mr. R. may at some future period be tempted to think, but sure am I that General B. can think no such thing, when he tells us from whom he got it. We may observe, too, that the method of observing both upper and under limbs has been long practised at Greenwich.

"Though the absolute identity," says Mr. R. [erroneous formula and all, I suppose] "in every particular is, to say nothing else, a very curious circumstance, I have no reason to believe that General B. has availed himself of any thing that I have done on the subject, notwithstanding the publication of my letter took place long before his communication was written." [General B.'s example is for November 8, 1817.]

We do not know what to make of this sentence. It is certain General B. never supposed it a new discovery, but one he had received from the continental observers, and is substantially the same as those detailed at large by Baron Zach; also in the *Base du Système metrique*; and by Delambre in his *Astronomie*, tome ii. page 247, 248, &c.; which creates some surprise that Mr. R. ever thought of claiming it as a discovery,—for General Brisbane does no such thing, but merely wishes to draw the attention of his countrymen to that method of observing. Mr. R.'s surprise at Dr. Brewster's conduct might perhaps be lessened, if he would look into the Edinburgh Encyclopædia, article REPEATING CIRCLE, we think (for we quote from memory), where he will find several tables the same as General Brisbane's, with their description and use in astronomical and geodetical observations. Dr. Brewster's object in both cases is to bring the method into more general practice in this country, though it is not absolutely new.

I have extended this letter far longer than the importance of the subject seemed to demand; for the assertions or insinuations contained in Mr. R.'s paper, can only mislead the ignorant, either real or intentional, but can have no effect whatever on the character of individuals blamed in it.

I have the honour to be, dear sir, yours,

To the Editor of the Phil. Mag. J.

VIII. *On the magnetic Phænomena produced by Electricity; in a Letter from Sir* H. DAVY, *Bart. F.R.S. to* W. H. WOLLASTON, *M.D. P.R.S.**

MY DEAR SIR,—THE similarity of the laws of electrical and magnetic attraction has often impressed philosophers, and many years ago, in the progress of the discoveries made with the Voltaic pile, some inquirers (particularly M. Ritter†) attempted to establish the existence of an identity or intimate relation between these two powers; but their views being generally obscure, or their experiments inaccurate, they were neglected: the chemical and electrical phænomena exhibited by the wonderful combination of Volta, at that time almost entirely absorbed the attention of scientific men; and the discovery of the fact of the true connexion between electricity and magnetism, seems to have been reserved for M. Œrsted, and for the present year.

This discovery, from its importance and unexpected nature, cannot fail to awaken a strong interest in the scientific world; and it opens a new field of inquiry, into which many experimenters will undoubtedly enter: and where there are so many objects of research obvious, it is scarcely possible that similar facts should not be observed by different persons. The progress of science is, however, always promoted by a speedy publication of experiments; hence, though it is probable that the phænomena which I have observed may have been discovered before, or at the same time, in other parts of Europe, yet I shall not hesitate to communicate them to you, and through you to the Royal Society.

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* From the Transactions of the Royal Society, for 1821, Part I.

† M. Ritter asserted that a needle composed of silver and zinc arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet, and that a metallic wire, after being exposed in the Voltaic circuit, took a direction N. E. and S. E. His ideas are so obscure, that it is often difficult to understand them, but he seems to have had some vague notion that electrical combinations, when not exhibiting their electrical tension, were in a magnetic state, and that there was a kind of electro-magnetic meridian depending upon the electricity of the earth.—See *Ann. de Chimie*, t. lxiv. p. 80. Since this letter has been written, Dr. Marcet has been so good as to send me from Genoa, some pages of Aldini on Galvanism, and of Izarn's Manual of Galvanism, published at Paris more than sixteen years ago. M. Mojon, senior, of Genoa, is quoted in these pages as having rendered a steel needle magnetic, by placing it in a Voltaic circuit for a great length of time. This, however, seems to have been dependent merely upon its place in the magnetic meridian, or upon an accidental curvature of it, but M. Romagnesi, of Trente, is stated to have discovered that the pile of Volta caused a declination of the needle, the details are not given, but if the general statement be correct, the author could not have observed the same fact as M. Œrsted, but merely supposed that the needle had its magnetic poles altered after being placed in the Voltaic circuit as a part of the electrical combination.

I found, in repeating the experiments of M. Œrsted with a Voltaic apparatus of one hundred pair of plates of four inches, that the south pole of a common magnetic needle (suspended in the usual way) placed under the communicating wire of platinum, (the positive end of the apparatus being on the right hand,) was strongly attracted by the wire, and remained in contact with it, so as entirely to alter the direction of the needle, and to overcome the magnetism of the earth. This I could only explain by supposing that the wire itself became magnetic during the passage of the electricity through it, and direct experiments, which I immediately made, proved that this was the case. I threw some iron filings on a paper, and brought them near the communicating wire, when immediately they were attracted by the wire, and adhered to it in considerable quantities, forming a mass round it ten or twelve times the thickness of the wire: on breaking the communication, they instantly fell off, proving that the magnetic effect depended entirely on the passage of the electricity through the wire. I tried the same experiment on different parts of the wire, which was seven or eight feet in length, and about the twentieth of an inch in diameter, and I found that the iron filings were every where attracted by it; and making the communication with wires between different parts of the battery, I found that iron filings were attracted, and the magnetic needle affected, in every part of the circuit.

It was easy to imagine that such magnetic effects could not be exhibited by the electrified wire without being capable of permanent communication to steel. I fastened several steel needles, in different directions, by fine silver wire to a wire of the same metal, of about the thirtieth of an inch in thickness and eleven inches long, some parallel, others transverse, above and below, in different directions: and I placed them in the electrical circuit of a battery of thirty pairs of plates of nine inches by five, and tried their magnetism by means of iron filings. they were all magnetic: those which were parallel to the wire attracted filings in the same way as the wire itself; but those in transverse directions exhibited each two poles, which being examined by the test of delicate magnets, it was found that all the needles that were placed under the wire (the positive end of the battery being east,) had their north poles on the south side of the wire, and their south poles on the north side; and that those placed over, had their south poles turned to the south, and their north poles turned to the north; and this was the case whatever was the inclination of the needles to the horizon. On breaking the connexion, all the steel needles that were on the wire in a transverse direction retained their magnetism, which was as powerful as ever, whilst those which were parallel to the silver wire appeared to lose it at the same time as the wire itself. Let-

I attached small longitudinal portions of wires of platinum, silver, tin, iron, and steel, in transverse directions, to a wire of platinum that was placed in the circuit of the same battery. The steel and the iron wire immediately acquired poles in the same manner as in the last experiment; the other wires seemed to have no effect, except in acting merely as parts of the electrical circuit; the steel retained its magnetism as powerfully after the circuit was broken as before; the iron wire immediately lost a part of its polarity, and in a very short time the whole of it.

The battery was placed in different directions as to the poles of the earth; but the effect was uniformly the same. All needles placed transversely under the communicating wires, the positive end being on the right hand, had their north poles turned towards the face of the operator, and those above the wire their south poles; and on turning the wire round to the other side of the battery, it being in a longitudinal direction, and marking the side of the wire, the same side was always found to possess the same magnetism; so that in all arrangements of needles transversely round the wire, all the needles above had north and south poles opposite to those below, and those arranged vertically on one side, opposite to those arranged vertically on the other side.

I found that contact of the steel needles was not necessary, and that the effect was produced instantaneously by the mere juxta-position of the needle in a transverse direction, and that through very thick plates of glass: and a needle that had been placed in a transverse direction to the wire merely for an instant, was found as powerful a magnet as one that had been long in communication with it.

I placed some silver wire of $\frac{1}{20}$ of an inch, and some of $\frac{1}{50}$, in different parts of the Voltaic circuit when it was completed, and shook some steel filings on a glass plate above them: the steel filings arranged themselves in right lines always at right angles to the axis of the wire; the effect was observed, though feebly, at the distance of a quarter of an inch above the thin wire, and the arrangement in lines was nearly to the same length on each side of the wire.

I ascertained by several experiments, that the effect was proportional to the quantity of electricity passing through a given space, without any relation to the metal transmitting it: thus, the finer the wires the stronger their magnetism.

A zinc plate of a foot long and six inches wide, arranged with a copper plate on each side, was connected, by a very fine wire of platinum, according to your method; and the plates were plunged an inch deep in diluted nitric acid. The wire did not sensibly attract fine steel filings. When they were plunged two inches, the effect was sensible; and it increased with the quantity of immersion. Two arrangements of this kind acted more powerfully than one; but when the two were combined so as to make the zinc and copper-plates but parts of one combination, the effect was very much greater. This was shown still more distinctly in the following experiment. Sixty zinc plates with double copper-plates were arranged in alternate order, and the quantity of iron filings which a wire of a determinate thickness took up observed: the wire remaining the same, they were arranged so as to make a series of thirty; the magnetic effect appeared more than twice as great; that is, the wire raised more than double the quantity of iron filings.

The magnetism produced by Voltaic electricity seems (the wire transmitting it remaining the same) exactly in the same ratio as the heat; and however great the heat of a wire, its magnetic powers were not impaired. This was distinctly shown in transmitting the electricity of twelve batteries of ten plates each of zinc, with double copper arranged as three, through fine platinum wire, which, when so intensely ignited as to be near the point of fusion, exhibited the strongest magnetic effects, and attracted large quantities of iron filings and even small steel needles from a considerable distance.

As the discharge of a considerable quantity of electricity through a wire seemed necessary to produce magnetism, it appeared probable, that a wire electrified by the common machine would not occasion a sensible effect; and this I found was the case, on placing very small needles across a fine wire connected with a prime conductor of a powerful machine and the earth. But as a momentary exposure in a powerful electrical circuit was sufficient to give permanent polarity to steel, it appeared equally obvious that needles placed transversely to a wire at the time that the electricity of a common Leyden battery was discharged through it, ought to become magnetic; and this I found was actually the case, and according to precisely the same laws as in the Voltaic circuit: the needle *under* the wire, the positive conductor being on the right hand, offering its north pole to the face of the operator, and the needle *above*, exhibiting the opposite polarity.

So powerful was the magnetism produced by the discharge of an electrical battery of 17 square feet highly charged, through a silver wire of $\frac{1}{20}$ of an inch, that it rendered bars of steel of two inches long and from $\frac{1}{20}$ to $\frac{1}{10}$ in thickness, so magnetic, as to enable them to attract small pieces of steel wire or needles; and the effect was communicated to a distance of five inches above or below or laterally from the wire, through water or thick plates of glass or metal electrically insulated.

The facility with which experiments were made with the common Leyden battery, enabled me to ascertain several circumstances which were easy to imagine, such as that a tube filled with sulphuric acid of a quarter of an inch in diameter, did not transmit sufficient electricity to render steel magnetic; that a needle placed transverse to the explosion through air, was less magnetized than when the electricity was passed through wire; that steel bars exhibited no polarity (at least at their extremities) when the discharge was made through them as part of the circuit, or when they were placed parallel to the discharging wire; that two bars of steel fastened together, and having the discharging wire placed through their common centre of gravity, showed little or no signs of magnetism after the discharge till they were separated, when they exhibited their north and south poles opposite to each other, according to the law of position.

These experiments distinctly showed, that magnetism was produced whenever concentrated electricity passed through space; but the precise circumstances, or law of its production, were not obvious from them. When a magnet is made to act on steel filings, these filings arrange themselves in curves round the poles, but diverge in right lines; and in their adherence to each other form right lines, appearing as spicula. In the attraction of the filings round the wire in the Voltaic circuit, on the contrary, they form one coherent mass, which would probably be perfectly cylindrical were it not for the influence of gravity. In first considering the subject, it appeared to me that there must be as many double poles as there could be imagined points of contact round the wire; but when I found the N. and S. poles of a needle uniformly attracted by the same quarters of the wire, it appeared to me that there must be four principal poles corresponding to these four quarters. You, however, pointed out to me that there was nothing definite in the poles, and mentioned your idea, that the phænomena might be explained, by supposing a kind of revolution of magnetism round the axis of the wire, depending for its direction upon the position of the negative and positive sides of the electrical apparatus.

To gain some light upon this matter, and to ascertain correctly the relations of the north and south poles of steel magnetized by electricity to the positive and negative state, I placed short steel needles round a circle made on pasteboard, of about two inches and a half in diameter, bringing them near each other, though not in contact, and fastening them to the paste-board by thread, so that they formed the sides of a hexagon inscribed within the circle. A wire was fixed in the centre of this circle, so that the circle was parallel to the horizon, and an electric shock was passed through the wire, its upper part being connected with the positive side of a battery, and its lower part with the negative. After the shock all the wires were found magnetic, and each had two poles; the south pole being opposite to the north pole of the wire next to it, and *vice versâ*; and when the north pole of a needle was touched with a wire, and that wire moved round the circle to the south pole of the same needle, its motion was opposite to that of the apparent motion of the sun.

A similar experiment was tried with six needles arranged in the same manner; with only this difference, that the wire positively electrified was below. In this case the results were precisely the same, except that the poles were reversed; and any body, moved in the circle from the north to the south pole of the same needle, had its direction from east to west.

A number of needles were arranged as polygons in different circles round the same piece of paste-board, and made magnetic by electricity; and it was found that in all of them, whatever was the direction of the paste-board, whether horizontal or perpendicular, or inclined to the horizon, and whatever was the direction of the wire with respect to the magnetic meridian, the same law prevailed: for instance, when the positive wire was east, and a body was moved round the circle from the north to the south poles of the same wire, its motion (beginning with the lower part of the circle) was from north to south, or with the upper part from south to north; and when the needles were arranged round a cylinder of paste-board so as to cross the wire, and a pencil mark drawn in the direction of the poles, it formed a spiral.

It was perfectly evident from these experiments, that as many polar arrangements may be formed as chords can be drawn in circles surrounding the wire; and so far these phænomena agree with your idea of revolving magnetism: but I shall quit this subject, which I hope you will yourself elucidate for the information of the Society, to mention some other circumstances and facts belonging to the inquiry.

Supposing powerful electricity to be passed through two, three, four, or more wires, forming part of the same circuit parallel to each other in the same plane, or in different planes, it could hardly be doubted that each wire, and the space around it, would become magnetic in the same manner as a single wire, though in a less degree; and this I found was actually the case. When four wires of fine platinum were made to complete a powerful Voltaic circuit, each wire exhibited its magnetism in the same manner, and steel filings on the sides of the wires opposite attracted each other.

As the filings on the opposite sides of the wire attracted each other in consequence of their being in opposite magnetic states, it was evident, that if the similar sides could be brought into contact, steel filings upon them would repel each other.—This was very easily tried with two Voltaic batteries arranged parallel to each other, so that the positive end of one was opposite to the negative end of the other: steel filings upon two wires of platinum joining the extremities strongly repelled each other. When the batteries were arranged in the *same* order, *i. e.* positive opposite to positive, they attracted each other; and wires of platinum (without filings) and fine steel wire (still more strongly) exhibited similar phænomena of attraction and repulsion under the same circumstances.

As bodies magnetized by electricity put a needle in motion, it was natural to infer that a magnet would put bodies magnetized by electricity in motion; and this I found was the case. Some pieces of wire of platinum, silver, and copper, were placed separately upon two knife edges of platinum connected with two ends of a powerful Voltaic battery, and a magnet presented to them; they were all made to roll along the knife edges, being attracted when the north pole of the magnet was presented, the positive side of the battery being on the right hand, and repelled when it was on the left hand; and *vice versâ*, changing the pole of the magnet. Some folds of gold leaf were placed across the same apparatus, and the north pole of a powerful magnet held opposite to them; the folds approached the magnet, but did not adhere to it. On the south pole being presented, they receded from it.

I will not indulge myself by entering far into the theoretical part of this subject; but a number of curious speculations cannot fail to present themselves to every philosophical mind, in consequence of the facts developed; such as whether the magnetism of the earth may not be owing to its electricity, and the variation of the needle to the alterations in the electrical currents of the earth in consequence of its motions, internal chemical changes, or its relations to solar heat; and whether the luminous effects of the auroras at the poles are not shown, by these new facts, to depend on electricity. This is evident, that if strong electrical currents be supposed to follow the apparent course of the sun, the magnetism of the earth ought to be such as it is found to be.

But I will quit conjectures, to point out a simple mode of making powerful magnets, namely, by fixing bars of steel across, or circular pieces of steel fitted for making horse-shoe magnets, round the electrical conductors of buildings in elevated and exposed situations*.

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* There are many facts recorded in the Philosophical Transactions which prove the magnetizing powers of lightning; one in particular, where a stroke

The experiments detailed in these pages were made with the apparatus belonging to the Royal and London Institutions; and I was assisted in many of them by Mr. Pepys, Mr. Allen, and Mr. Stodart, and in all of them by Mr. Faraday*.

I am, my dear sir,
Very sincerely yours,
Lower Grosvenor-street, Nov. 12, 1820. HUMPHRY DAVY.

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of lightning passing through a box of knives, rendered most of them powerful magnets. See Philosophical Transactions, No. 157, p. 520; and No. 437, p. 57.

* All the experiments detailed in this paper, except those mentioned p. 18, were made in the course of October 1820, the last arose in consequence of a conversation with Dr. Wollaston, and were made in the beginning of November. I find, by the *Annales de Chimie et de Physique*, for September, which arrived in London November 24, that M. Arago has anticipated me in the discovery of the attractive and magnetizing powers of the wires in the Voltaic circuit, but the phænomena presented by the action of common electricity (which I believe as yet have been observed by no other person), induce me still to submit my paper to the Council of the Royal Society. Before my notice arrived of the researches of the French philosophers, I had tried, with Messrs. Allen and Pepys, an experiment, which M. Arago likewise thought of,—whether the arc of flame of the Voltaic battery would be affected by the magnet, but from the imperfection of our apparatus, the results were not decisive. I hope soon to be able to repeat it under new circumstances.

I have made various experiments, with the hope of affecting electrified wires by the magnetism of the earth, and of producing chemical changes by magnetism; but without any successful results.

Since I have perused M. Ampere's elaborate treatise on the electro-magnetic phænomena, I have passed the electrical shock along a spiral wire twisted round a glass tube containing a bar of steel, and I found that the bar was rendered powerfully magnetic by the process.

Without meaning to offer any decided opinion on that gentleman's ingenious views, I shall beg permission to mention two circumstances, which seem to me unfavourable to the idea of the identity of electricity and magnetism, 1st, the great distance to which magnetism is communicated by common electricity (I found that a steel bar was made magnetic at fourteen inches distance from a wire transmitting an electric shock from about seventy feet of charged surface), and, 2d, that the effect of magnetizing at a distance by electricity takes place with the same readiness through air and water, glass, mica, or metals, *i.e.* through conductors and non-conductors.

IX. *True apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. GROOBY.

[Continued from vol. lvii. p. 408.]

1821	γ Pegasi	α Arietis	α Ceti	Aldebaran	Capella	Rigel	β Tauri	α Orionis	Sirius	Castor	Procyon	Pollux	α Hydrae	Regulus	β Leonis	β Virginis	Spica Virginis	Arcturus
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 16	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
Sept.	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s
1	5 53	9 78	59 11	42 53	32 29	58 70	2 02	31 49	17 04	12 32	57 62	23 38	48 80	51 53	57 05	22 85	49 22	31 92
2	55	81	14	57	34	73	05	52	07	35	65	40	82	54	05	87	20	91
3	56	84	17	51	38	76	09	55	10	38	67	43	83	55	05	89	18	90
4	58	86	20	64	43	79	13	58	13	41	70	46	85	57	05	91	16	88
5	59	89	22	67	47	82	16	61	16	44	72	49	86	58	06	93	14	87
6	61	91	25	70	51	85	20	64	19	48	75	52	88	59	06	94	13	86
7	62	94	28	71	56	88	24	67	22	51	77	55	90	61	06	96	11	85
8	63	95	30	77	60	90	28	70	25	54	80	58	91	62	07	97	10	84
9	64	99	32	80	65	93	31	73	27	57	82	61	93	64	07	98	09	83
10	65	10 01	35	83	69	96	35	76	30	61	85	64	95	65	08	99	09	82
11	67	03	37	86	71	99	38	79	33	64	88	67	97	67	08	23 00	10	81
12	68	05	39	89	75	59 02	41	82	36	67	91	70	98	68	09	00	10	80
13	69	08	41	92	80	05	45	85	39	70	93	73	49 00	70	10	01	11	79
14	79	10	43	95	86	08	48	87	42	73	96	76	02	72	10	02	11	78
15	71	12	45	98	91	11	52	90	45	77	98	79	04	73	11	02	11	77
16	72	15	48	43 01	95	14	55	94	47	80	58 01	83	06	75	11	03	12	76
17	73	17	50	05	99	17	59	97	50	83	04	86	08	77	12	04	12	75
18	74	19	53	08	33 03	19	62	32 00	53	86	07	88	10	78	13	05	12	74
19	75	21	55	11	08	22	66	03	55	90	10	92	12	80	13	06	13	74
20	76	23	58	14	12	25	69	06	58	93	12	95	14	82	14	06	13	73
21	76	26	60	17	16	28	73	09	61	97	15	98	16	84	15	07	13	72
22	77	28	62	20	20	31	76	13	64	13 00	18	24 02	19	86	16	08	13	71
23	78	30	65	23	25	34	79	16	67	03	21	05	21	88	17	08	14	71
24	79	32	68	26	29	37	83	19	70	07	23	08	23	90	18	09	14	70
25	80	35	71	29	33	40	86	22	73	10	26	11	25	91	18	10	14	69
25	80	37	73	31	37	42	90	25	76	13	28	14	27	93	19	11	14	69
27	81	39	76	[illegible]	42	45	93	28	78	17	31	18	30	95	20	12	15	68
28	81	41	78	37	46	48	96	31	81	20	33	21	32	97	21	13	15	68
29	82	42	80	40	50	51	3 00	34	84	23	38	25	34	99	22	14	15	67
30	83	45	83	43	54	53	03	37	87	27	41	28	36	52 01	23	15	16	67

1821.	1α Libræ	2α Libræ	α Cor. Bor.	α Serpentis	Antares.	α Herculis	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ	β Aquilæ.	1α Capri	2α Capri	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Sept.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	50 40	1·83	8 84	29 85	30 88	32 11	40 61	55 37	48 41	0 43	34 76	47 14	10 89	22 97	39·17	48 85	54 85	13 01
2	38	82	82	84	87	10	59	34	40	42	75	13	88	96	18	86	86	03
3	37	81	81	82	85	08	58	32	39	41	74	12	87	95	18	87	87	05
4	36	80	79	81	83	06	56	30	38	40	73	12	87	94	18	88	88	06
5	35	79	77	80	82	05	55	28	37	39	72	11	86	92	19	89	88	08
6	34	78	75	78	80	03	53	26	36	38	72	11	86	91	19	90	89	09
7	33	77	74	77	79	02	52	24	35	37	71	10	85	90	19	91	90	11
8	32	76	72	75	77	00	50	22	34	36	70	09	84	88	19	91	90	12
9	31	75	70	74	76	31 98	48	19	33	34	69	08	83	87	19	92	91	13
10	30	74	69	72	74	97	47	17	31	33	68	07	82	85	19	92	91	15
11	29	73	67	71	73	95	45	15	30	32	66	06	81	84	19	93	92	16
12	28	72	65	69	71	93	43	13	29	31	65	05	80	82	19	93	92	17
13	28	72	63	68	70	91	42	10	28	30	64	04	79	80	18	94	92	18
14	27	71	61	67	68	90	40	08	27	28	63	04	79	79	18	94	93	19
15	26	70	60	65	67	88	38	06	25	27	61	03	78	77	18	94	93	20
16	25	69	58	64	65	86	37	04	24	26	60	02	77	75	18	95	93	21
17	24	68	57	62	64	85	35	01	23	25	59	01	76	73	18	95	94	22
18	23	67	55	61	62	83	33	54 99	21	23	58	00	75	72	18	95	94	23
19	22	66	53	60	61	81	31	96	20	22	56	46 98	73	70	17	95	94	23
20	21	65	52	59	60	79	30	94	18	21	55	97	72	68	17	95	94	24
21	20	64	50	57	58	78	28	92	17	19	53	96	71	65	17	95	94	25
22	19	63	49	56	56	76	26	89	15	18	52	95	70	64	16	95	94	25
23	18	62	47	55	55	74	24	87	14	17	51	94	69	62	16	94	94	26
24	18	62	45	54	53	72	23	84	12	15	49	92	67	60	15	94	94	27
25	17	61	44	53	52	71	21	82	11	14	48	91	66	58	15	94	94	28
26	16	60	43	51	50	69	19	79	09	13	46	90	65	56	14	94	94	28
27	15	59	41	50	49	67	17	77	08	11	45	89	64	54	14	94	94	29
28	14	58	40	49	48	66	16	75	06	10	44	88	63	52	13	93	94	29
29	14	58	39	48	46	64	14	72	05	09	42	86	61	50	13	93	94	29
30	13	57	37	47	45	62	12	70	03	07	41	85	60	48	12	93	93	30

X. *On the Glow-worm.* *By Mr.* W. ROGERSON, *Jun.*

Pocklington, June 20, 1821

SIR,—THE following remarks on that curious insect called the Glow-worm, are founded on my own experience.—If you think they will be entertaining or useful to any of your readers, they are at your service.

The glow-worm is an insect of the beetle kind:—the female deposits her eggs in the months of June or July, among moss, grass, &c.: these eggs are of a yellow colour, and emit light. After remaining about five or six weeks, the larvæ break the shells and make their appearance: at first they appear white, and are very small; but they soon increase in size, and their colour changes to a dark brown or nearly black colour. The body of the larva is formed of eleven rings; it has six feet, and two rows of reddish spots down the back. It emits light in the dark; this light arises from the last ring of its body under the tail, and appears like two brilliant spots when examined attentively.

The larvæ are seen creeping about and shining during the fine nights in autumn, and the light they emit is to direct them to their food: they feed on small snails, the carcases of insects, &c. They frequently cast off their skins.

After the expiration of about one year and nine months from their birth, they arrive at their perfect size—they then cease to eat, cast off their skin, and assume another appearance: the form of the perfect insect may be discovered through a thin skin that covers them. After remaining in this state two or three weeks (scarcely ever moving) they throw off their last skin, and arrive at perfection. The male then appears a perfect beetle, having wings, and covers to the same. The female, on the contrary, has neither wings nor wing cases: she is larger than the male, and of a lighter colour. It is the female that principally shines in the perfect state: her light is far superior to that emitted by the larva, and arises from the three last rings of the body on the lower side. The reason why the female shines, I am assured from repeated experiences which I have made, is to allure the male to her company.

Here we behold the wonderful wisdom of the great Creator; she, being void of wings, therefore incapable of flying through liquid air to seek her mate, is provided with a beautiful lamp which answers her purpose as well. After the female has been impregnated by the male, she deposits her eggs, and dies. The male dies also.

Those who wish to investigate this curious insect, may keep them in glass jars, among damp moss, and feed the larvæ with snails cut into pieces. I have kept glow-worms for years in glasses, and have traced them through all the changes of their lives from their exclusion from the egg to their death.

To the Editor of the Phil. Mag. W. ROGERSON, Jun.

XI. *Remarks and Suggestions, as to the State and Progress of the Government Trigonometrical Survey, with regard to the Dimensions, Figure and Structure of the Earth.* By Mr. JOHN FAREY, Sen.

To the Editor.

SIR, — A SELECT Committee of the House of Commons, on *Weights and Measures*, after considering the three several Reports* of the late Government Commissioners on the same subject, have made a Report to the House (which was ordered to be printed on the 28th of May, and which doubtless will find a place in your present or some early Number†) in which Report, after a well-merited compliment paid to Capt. Kater, for his elaborate and gratuitous Experiments on the Pendulum, in London, and for similar Observations on the principal Stations of the Trigonometrical Survey of Great Britain, the Commissioners remark as follows, viz. "From these observations, deductions have been made, of great importance with respect to the general figure of the Earth, its density and internal construction. So that your Committee are decidedly of opinion, that it will be highly proper to extend similar Observations over a still larger surface, so as to connect the measurements and astronomical observations made by the different Nations of Europe, as much as possible, into one whole."

I am sorry not to be able to concur with the Committee in thinking, that deductions of any great importance, as to the exact *figure* or *structure* of the Earth, have yet resulted from the Trigonometrical Survey of these Islands, or that much, if any, of the wished-for information on these points would be derived, from *more widely* extending the Pendulum observations, until after such Mineralogical or *stratigraphical Surveys* and investigations shall have been made in England, as I have in your 48th volume, p. 130, recommended, around the Stations, where already the Standard Pendulum has been swung; *Arbury Hill* in particular.

It appears to me also essential, that most or all of the several Observations that I have recommended in the volume quoted, including those of the Pendulum, should be very carefully made, and repeated, at so many others of the British Stations, as to be able to institute rigid calculations, of the lengths of degrees of Latitude, on two or three other Meridians, besides that already calculated, which passes through *Dunnose* in the Isle of Wight; and also degrees of Longitude, (or else of great circles perpendicular to some particular meridian) in several different Latitudes: in order, that by the consistency and agreement or otherwise, amongst the results, with the known fact of the Earth nearly approaching in figure to an *Ellipsoid* slightly flattened at the Poles, it may be seen, and duly appreciated, what degree of dependence can be placed on the methods of observations and calculations, which hitherto have been adopted or recommended.

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* The First of these Reports will be found in p 172, of our 54th volume, and the Third of them, in p. 359 of our 57th volume, and its Appendix in p. 120; the intermediate Report, as well as two Appendixes, detailing the incongruous mass of *legal* provisions existing on this subject, and the still more incongruous and numerous *local denominations* of Measures and Weights *in use*, will doubtless ere long be printed in a separate form.—EDITOR.

† See our Number for June, p 432.—EDIT.

In particular I am anxious, that the District around and to the northward of *Arbury* Hill, wherein Capt. Kater has concluded, that *a mass of the Stratification of great comparative specific Gravity*, must be situated, at no great depth below the surface, should be surrounded and crossed by several lines or degrees of latitude and of longitude, and by those also of oblique Arcs or Rhombs, between the several Stations surrounding the district under investigation: from which, and the proposed minute Stratigraphical Surveys, to try the practicability, of consistently deducing the *magnitude, shape, position and specific Gravity* of the supposed heavy or *deflecting Mass*, which is assumed to have occasioned such a deflection of the plumb-line at *Arbury Hill* Station, as to have presented the anomaly, of degrees of the Meridian, increasing in length, in the contrary direction to those of the Ellipsoid, above alluded to: and whether or not, *a mass of Granite*, the top of which presents itself at *Mount Sorrel, Grooby*, and other places * in and near to the south part of Charnwood Forest, is sufficient to account wholly, or in considerable part, for the deflection alluded to.

Until the facts as to our own country shall have been settled beyond dispute or doubt; or at least, until the attempt shall seriously have been made, to explain or remove the Arbury-Hill anomaly in the lengths of meridional Degrees, it would, I submit to your Readers, be premature, and likely to perpetuate rather than to remove errors, if the Observations, merely as hitherto conducted, and without going more deeply into the subject, were extended to the other countries of Europe, with the view, as the Committee appear to propose, of thereby coming to definitive conclusions as to the dimensions, figure and structure of the Earth.

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* See my Derbyshire Report, vol. i p. 151: observing, that this was written, before *the unconformableness* of numerous local parts of the English stratification, had been made out, or the effects of such unconformableness sufficiently considered by myself or any other Writer. See P. M. vol. xliii. p 330 Note, vol. xlv. p. 169, &c.

Entertaining as I do, a high respect for the person, abilities and labours of Dr. MacCulloch, and not being by any means desirous of undervaluing the Services to Mineralogy and Geology which he has performed, and on which I understand him to be yet engaged *in Scotland*, and with respect to *a mineralogical Map* and description of that Kingdom, as is briefly mentioned in p. 228 of your 56th volume, I hope and trust, that what I am going further to remark, may not give offence to that Gentleman or any of his Friends. It was naturally to be expected from the announcement made in 1816, which is quoted in p. 427 of your 48th volume, that the making of minute *stratigraphical Maps and Sections* around each principal Station of the Trigonometrical Survey, in Scotland, at the least, was then intended: I have not however yet been able to learn, that any such *materials for calculation*, as to the existence and extent of local deflecting causes on the plumb-line in Scotland, have resulted from Dr. MacCulloch's appointment; or that anything has been attempted of this kind in England, by him or any other person.

If Government have seen it right to devote a part of the public Money to the making of a Mineralogical Survey and Map of Scotland, (in addition to the sums devoted to its Canals, Bridges, Roads, &c.), it cannot surely wish to withhold the means, of completing the Trigonometrical Survey of England, and of Wales and Scotland, which it has so long and laudably supported, in those remaining points connected with the Stratification, on which evidently so much of the minute accuracy of the whole is dependent, with reference to the dimensions, figure and structure of the Earth.

Let me presume to hope, that the present season of Peace, when Expeditions are so liberally fitted out and appointed, for exploring distant regions, and when so many public Works are carrying on, may not pass away, without the services to Science being performed, which I have ventured to suggest:—I am not like Captain Kater, able to offer gratuitous assistance to any considerable extent; but as far as prudence can warrant, I would be ready to co-operate in the stratigraphical Surveys and investigations recommended, if through the medium of any friends of Science amongst your Readers, such recommendation should be approved and acted on, by that department of Government to which it belongs.

I am, your obedient servant,

John Farey *Sen.*

37, Howland-street, Fitzroy-sq. &c.
June 17, 1821

P. S.—Mr. **William Smith**, after the considerable interruption to the publication of his series of *County Geological Maps*, which has originated in the unhandsome treatment received, in the quarter from whence he should have expected effective patronage, has completed his Map of *Yorkshire*, in four sheets, and the same is now in course of delivery by *Cary* of St. James's street: this Map, from embracing almost the whole series of the British Strata, and from the ample details which it contains, cannot fail of being acceptable to Land and Mineral Owners, and to all those anxious to become acquainted with the structure of the very interesting and valuable part of our Island which it embraces.

XII. *Hints for the approaching Harvest.*

A residence of two years in Switzerland, and particular attention directed to the rural œconomy of the country, amongst other things brought me acquainted with a method of harvesting which to me was quite new. Since my return home I have made many inquiries without having learned that a similar method was ever practised in England, though it is by no means improbable that it may already be known, either in local custom of old date, or from having been introduced by travellers who had equal opportunities with myself of observing what passed before their eyes. I am not aware, however, that any written description of this method has already appeared: and it is under a persuasion of its great utility that I now endeavour to give it every possible publicity.

In harvesting, two important matters present themselves to the consideration of the farmer:—expense of labour, and time. A saving in *labour*, if it protracts the time, is rarely beneficial, as it exposes the crop to additional risk and accident from bad weather; but every saving in *time* is a positive advantage.—The Swiss method saves both time and labour; and the loss from shedding, in handling the corn, appears to be less than with us. The main principle of their system consists in the use of the *scythe* instead of the *sickle*. Every one must be aware of the very superior powers of the former instrument; but in the ordinary mode of using it, notwithstanding the various contrivances of bows, sweeps, cradles, &c which have been attached to the scythe to remedy the inconvenience, the corn is thrown down in such rude heaps, and the ears become so entangled, that a great loss follows from shedding: and hence farmers have conceived it better to employ six sickles than one scythe. Nay, I have heard it argued, when prices were high, that there was good œconomy in *reaping* even barley.

In Switzerland, however, by a very simple contrivance the objections to the scythe have been completely obviated. Each mower is accompanied by a person bearing a light straight long pole, whose business it is to press the pole in a horizontal position against the stems of the corn which are to be cut, so as to bend the ears considerably away from the mower, and leave him a full, fair, stroke at the aggregated stems. The pole is best made of deal, much about the thickness of the handle of a sweeping-brush, and in Switzerland is commonly about nine feet in length; but this must be determined by the nature of the scythe used*, or rather by the sweep or extent of the stroke: for the person who bears the pole must be out of the reach of all danger from the scythe, and at the same time there must be sufficient length of pole left to press against the whole breadth or *bench* of standing corn which the mower can compass with his scythe. The person who bears the pole, commonly a lad, stands with his face towards the mower, a little to the left of his line of progression, and moves backwards as the other advances. The pole may be held as the bearer finds most convenient; and steadied, either against his body, or his thigh, according to his height: but care must be taken that it is held nearly horizontally, and the pole should press on the standing corn, from about six inches to a foot below the ears, so as to give a sloping direction, away from the mower, to the *whole* quantity likely to be cut at one stroke. The ears which are thus pressed back never straggle, either during or after the stroke, but slide along the pole; which, if properly held, keeps them quite even, and they will fall one way, smoothly and regularly.

Should the corn have been beaten down by rain or storm, the scythe cannot certainly be used with the same advantage as if it stands erect and healthy; the sickle will answer best under such circumstances. The Swiss, however, are so careful about their crops, that when fallen, they raise them, in part, by fastening the stems together, and tying them in a direction contrary to that in which they have fallen. Sometimes they plant stakes through the field, and put small cross bars or rods, from stake to stake, so as to bear the stems up; and after this the corn may be mowed. But in the case of a fine, even, upright crop, whether of wheat or other grain, there can be no hesitation in preferring the *scythe and pole*, if properly used, to the sickle.

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* The scythes commonly used in Switzerland have light, short blades, and I believe are principally of German manufacture. The handles are much bent; and the mower stands tolerably upright The sweep is not very great. But the Swiss are admirable mowers I have seen patches of grass on the Alps, growing under ledges of rock where no cattle could climb, cut as close and smooth as a dexterous English gardener could shave a grass plot. They put an exquisite edge on their scythes by hammering them out on little anvils kept for the purpose, instead of thinning the edge by a coarse stone as our mowers do; and their scythes by this treatment consequently last much longer. The operation is performed once in a day or two; and the edge is afterwards still further sharpened by a sort of strap or prepared board, finer than ours.

After the mower has passed on, two or more women or boys, commonly women, follow, whose business it is gently and carefully to spread the corn that has been mowed, much in the same way that flax or hemp is spread, in even regular layers. When properly dried, a third operation preparatory to binding is performed by two other persons, commonly women, one of whom carries two wands or rods, about four feet in length, the other one rod only. The person carrying the two rods, thrusts them, one in each hand, under the layer of corn which lies spread out on the ground; the person with the single wand, at the same time standing opposite to the other, puts the single wand under the layer, directly midway between the two opposite wands. Then the hands are raised, whilst the points of the rods or wands remain on the ground, and thus the corn slides, or is shoved together, in heaps large enough for binding. If the crop is thin, the gatherings must begin the wider apart, or two gatherings may be united, and great attention during this operation is paid to collecting any scattered or straggling stems. The same operation may be performed with a long-toothed rake; but the corn is shaken much less when the wands are used.

The binders follow those who have collected the heaps: and it is to be particularly noted, that the Swiss do not use the fresh cut corn for bands, but make them at home in the barns, of straw, at some convenient time in advance. When wanted for use, they are carted to the field, and distributed along the rows. The time spent in knotting the bands in the fields is thus saved, and the loss of grain from twisting the bands or rubbing against other sheaves in carrying home is avoided.

The advantages derived from this method, and the division of labour, will be obvious to any person who has once seen the perfect practice; and I do not think there is any exaggeration in stating, that with the same hands, a crop may by such means be saved in half the time. But I am inclined to go further, and express my conviction, that the crop may be saved in half the time with even *fewer hands* than the sickle requires. Much however will certainly depend on the dexterity and willingness of the people employed; but the mere experiment of the *scythe and pole* may be tried without any expense or difficulty.

In describing this method as Swiss, it is necessary to add, that it is by no means general throughout Switzerland, but confined at present to particular districts: after being once introduced,

however,

however, it is never abandoned; and I have been told it is spreading very rapidly through the country. There can be no surer proof of its excellence; for in a country exposed to calamities from inclement seasons, and where years of scarcity are by no means uncommon, it would certainly not be adopted, if it did not ensure to the husbandman the utmost possible produce from his crop. There is no want of hands in Switzerland; on the contrary, the valleys of Oberland, where I have observed the use of the scythe most prevalent, swarm with inhabitants; and men, women, and children all work in the fields with the greatest assiduity, both early and late

I have purposely delayed this publication until the time of harvest was near, under a persuasion that it was likely to produce more effect, when there was an opportunity of trying the experiment whilst the subject was fresh in the mind.

London, July 16, 1821. ISAAC WELD, jun.

XIII. *Notices respecting New Books.*

THE Philosophical Transactions for 1821, Part I. has just made its appearance, and the following are the contents:

I. On the black Rete mucosum of the Negro being a Defence against the scorching Effect of the Sun's Rays. By Sir E. Home, Bart. F.R.S.—II. On the magnetic Phænomena produced by Electricity; in a Letter from Sir H. Davy, Bart. F.R.S. to W. H. Wollaston, M.D. P.R.S.—III. A Communication of a singular Fact in Natural History. By the Right Hon. the Earl of Morton, F.R.S.; in a Letter addressed to the President.—IV. Particulars of a Fact, nearly similar to that related by Lord Morton, communicated to the President in a Letter from Daniel Giles, Esq. —V. The Croonian Lecture. Microscopical Observations on the following Subjects. On the Brain and Nerves; showing that the Materials of which they are composed exist in the Blood. On the Discovery of Valves in the Branches of the Vasa brevia, lying between the villous and muscular Coats of the Stomach. On the Structure of the Spleen. By Sir Everard Home, Bart. V.P.R.S.—IV. On two new Compounds of Chlorine and Carbon, and on a new Compound of Iodine, Carbon, and Hydrogen. By Mr. Faraday, Chemical Assistant in the Royal Institution. Communicated by W. T. Brande, Esq. Sec. R.S. and Prof. Chem. R.I.—VII. An Account of the Comparison of various British Standards of linear Measure. By Capt. Henry Kater, F.R.S.—VIII. An Account of the urinary Organs and Urine of two Species of the Genus Rana. By John Davy, M.D. F.R.S. IX. An Account of a Micrometer made of Rock Crystal. By G. Dollond, F.R.S.—X. The Bakerian Lecture. On the best Kind of Steel and Form for a Compass Needle. By Capt. Henry Kater, F.R.S. —XI. Notice respecting a volcanic Appearance in the Moon, in a Letter addressed to Sir Humphry Davy, Bart., P.R.S. By Capt. Henry Kater, F.R.S.—XII. A further Account of Fossil Bones discovered in Caverns inclosed in the Limestone Rocks at Plymouth. By Joseph Whidbey, Esq. In a Letter addressed to Sir Everard Home, Bart. V.P R.S.—XIII. On the aeriform Compounds of Charcoal and Hydrogen; with an Account of some additional Experiments on the Gases from Oil and from Coal. By William Henry, M.D. F.R.S. &c.

Report of the Select Committee on the Ophthalmic Hospital. pp. 8. 8vo.

It is with much pleasure we have perused this Report of a Select Committee of the House of Commons, on a branch of the public service which has of late been the object of much jealousy, and, we must add, of much calumny and misrepresentation. It is drawn up with great clearness and ability; and it does merited justice to the exertions of the individual whose discoveries, or at least superior intelligence and skill, gave rise to and placed him at the head of the Ophthalmic Establishment.

"The objects of this institution," says the Committee, "have been stated to us to be three.

"First, To diffuse, generally, among the surgeons of the army, the knowledge of the best modes of treating the chronic and third stage of the disorder.

"Secondly, To diminish, if possible, the charge of the out-pensioners of Chelsea Hospital, by curing or relieving men who had received pensions for defective sight.

"Thirdly, To check in some degree the annual augmentation of the pension list, by treating men about to be discharged for defective sight, and by thus diminishing their claim to pension, as far as it might be founded upon the impaired state of their vision.

"Your Committee are of opinion that these objects were of sufficient importance to justify the steps which were taken for their attainment.

"With respect to the first point, your Committee have the satisfaction to find, that this, which was the most important object, has been greatly promoted. The ophthalmia having upon the return of our troops from Egypt become, comparatively speaking, a new disease in this country, its proper treatment was at first imperfectly understood. It appears, however, that the attention of the Medical Department of the army has of late years been most successfully directed to this subject, and that the best modes of treating all the different stages of the ophthalmia are now well understood and practised in the army; and your Committee [illegible] the [illegible] ulation and attention of other practitioners.

"With respect to the second point [illegible], it has been stated, that valid doubts were suggested, how far it was in the power of the Commissioners of Chelsea Hospital to take away, or diminish, any pension which they had granted under the provisions of the act of the 46 Geo. III.; and consequently, your Committee have not thought it necessary to direct their inquiries to this point, as no diminution of pension already granted could, under any circumstances, have been effected.

"With respect to the third point, as but a few men so circumstanced have been placed in the Hospital, it does not appear to your Committee, that Sir William Adams has had sufficient opportunity of showing how far he could have effected this object, upon the scale originally proposed. But the general diffusion of knowledge among the medical officers of the army, must necessarily lead to the accomplishment of this end.

"With regard to the future continuance of this establishment, it has been stated to your Committee by the Department with which it originated, that the main objects for which it was instituted having thus been attained, it does not appear that any public inconvenience would now arise from its discontinuance. In this opinion your Committee are disposed to concur, and they therefore recommend, that the Establishment should be discontinued, as soon as the proper arrangement can conveniently be effected.

"Upon the claims of Sir William Adams upon the public, your Committee have to report, that he has rested those claims upon two grounds.

"First, upon his having been the means of promulgating to the army, and to the public, certain information as to the third or chronic stage of the ophthalmia and its consequences; namely, that it is the general, if not invariable, effect of the inflammation in the acute stage of the disorder, to produce in a greater or less degree, what are termed granulation on the inner surface of the eye-lid; that these granulations render the patient subject to relapses, and are frequently the cause of blindness; that during the relapses so happening, the patient is liable to become again infectious; and therefore, that these granulations must invariably be looked for, and removed, before the patient can be effectually cured.

"Secondly, upon his having attended the Ophthalmic Hospital since its first formation in 1817, without having hitherto received any remuneration for that duty.

"Upon the first point, your Committee have to report, That the existence of these granulations, and the necessity of removing them, seem to have been known in very early times, and are adverted to in the works of Celsus in the first century, of Paulus of Ægina in the seventh, of Rhases the Arabian in the tenth, and in the work of Sir William Reid in the reign of Queen Anne. That consequently no person in the present day, can claim more than the merit of having revived knowledge which had fallen into neglect. Your Committee do not feel it necessary to pronounce between the conflicting claims upon this head, or, by recommending a parliamentary reward for such revival, to decide to whom the merit properly belongs. They conceive, that question is best left to the decision of the profession, and of the public. But they are of opinion that Sir William Adams has, among others, been greatly instrumental in promulgating this knowledge, and in rendering it generally available.

"Upon the second point your Committee have to report, that since the first establishment of the Hospital in 1817, Sir William Adams has devoted to the duties arising out of his appointment, a large portion of that time, which to a professional man is the source of income; and that, inasmuch as the time which he could apply to his private practice has thereby been much curtailed, his professional emoluments must also have been proportionally lessened. That he has performed the difficult duties of his appointment with the greatest zeal and assiduity; and that your Committee have been led to form the highest opinion of his skill and abilities as an oculist.

"Your Committee taking into consideration all the circumstances of the case, are induced to recommend, that the sum of four thousand pounds should be paid to Sir William Adams, as a reward for the services which he has rendered to the public."

July 3, 1821.

Observations on certain Affections of the Head commonly called Head-aches; with a View to their more complete Elucidation, Prevention, and Cure; together with some brief Remarks on Digestion and Indigestion. By James Farmer, Member of the Royal College of Surgeons in London, and Licentiate of Midwifery of the Royal College of Physicians, Dublin. 18mo. 2s.

The Quarterly Journal of Foreign Medicine and Surgery and of the Sciences connected with them. No. XI. 8vo. 3s. 6d.

Preparing for Publication.

Dr. Conquest will publish, in a few weeks, a second and enlarged Edition of his "Outlines of Midwifery," &c. with copper-plate instead of lithographic Engravings. 12s.

Mr. Stevenson, Oculist and Dentist to His Royal Highness the Duke of York, &c. will shortly publish a practical Treatise on the Nature, Symptoms and Treatment of Gutta Serena, a Species of Blindness, arising from a Loss of Sensibility in the Nerve of Vision. Illustrated by numerous Cases.

Alexander Jamieson, Author of a Treatise on the Construction of Maps, and a Grammar of Geography and Elementary Astronomy, has now in the press a Celestial Atlas, being an exact Representation of the starry Firmament, as it appears to the Eye of an Observer on the Earth. This Work comprises general Constructions of the Hemispheres and Zodiac, with particular Projections of the successive Constellations from Pole to Pole, in Thirty Copper-plate Engravings. Each Plate is accompanied by a scientific Description of its Contents. The Method of finding the Place of the Constellation is also pointed out, and such Problems as are usually performed on the Celestial Globe, and may likewise be solved by Maps, are given as practical Examples for the Astronomical Student. And it is further illustrated by a Catalogue of the Stars it contains, from the first to the seventh Magnitude inclusive, indicated by Tables of their Right Ascension and Declination, with such other Notices of Celestial Phænomena as are most worthy of observation.

Religiosa Philosophia; or, A new Theory of the Earth, in Unison with the Mosaic Account of the Creation; illustrating, that by the Creator's command the Earth was formed in a Globe of Water, from whence it has arisen as a Tree from its Germ, and that the Doctrine of *Chaos* is founded in a Misconception of the meaning intended by the Sacred Historian. With an Appendix on the Plurality of inhabited Worlds. By W. Welch, of Stonehouse, Devon.

> “And God said, Let there be a Firmament in the midst of the waters And God made the Firmament, and divided the waters which were under the Firmament, from the waters which were above the Firmament.”
> *Gen. chap. i. ver. 6 and 7.*

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LITERATURE,

Instituted under the Patronage, and endowed by the Munificence of His Majesty King George the Fourth, for the Promotion of general Literature; to consist of a President, Vice-Presidents and Council; Fellows, Associates, and Honorary Members.

Origin and Endowment of the Society.

An accidental conversation which took place in October 1820, on the advantages which might be expected from the institution of a Society of Literature, somewhat resembling the French Academy of Belles Lettres, having been communicated to Sir Benjamin Bloomfield, and by him to The King;-and His Majesty having expressed his approbation of the proposal; a general outline of the Institution was by the Royal command submitted to His Majesty's perusal. On the 2d of November, the person who in conversation suggested the proposal received His Majesty's commands to attend His Majesty at Carlton House, for the purpose of digesting the best mode of giving effect to the undertaking; and was intrusted by His Majesty with full liberty to arrange the Plan of the Society. The Institution having, in its origin, no connexion with politics, or party of any kind, no applications were made to His Majesty's Ministers for their countenance or support, though its origin and progress were respectfully communicated to the Secretary of State for the Home Department.

Learning is, by principle, comprehensive and liberal in its views; and though the higher branches of literature have a natural connexion with peace, loyalty, and established order; yet, as the Founder and Patron of this Society, the King presents himself to his people, singly, as the friend of letters, as an example of munificence, and the promoter of what has been long wanting to the literary credit of the country.

His Majesty having been pleased to express, in the most favourable terms, his Royal approbation of this Society; and having honoured it with his munificent patronage, by assigning the annual sum of one hundred guineas each, for ten Associates, payable out of the Privy Purse; and also an annual Premium of one hundred guineas for the best Dissertation on some interesting subject, to be chosen by the Council of the Society:—

The following Regulations have been adopted as the basis of its proceedings.

Objects of the Society.

The objects of the Society are, to unite and extend the general interests of literature; to reward literary merit by patronage; to excite literary talent by premiums; and to promote literary education by bestowing exhibitions at the universities and public schools, in cases of distinguished desert.

Constitution of the Society.

§ 1. The Fellows constitute the principal body of the Society, and contribute to its support by subscriptions and benefactions. Every person elected a Fellow of this Society, shall pay annually the sum of two guineas, or more, at their option, or a proportional composition in lieu of the annual payments; and no person can be proposed for election unless on the recommendation of at least three Fellows.

§ 2. The Associates form that portion of the Society to which its patronage is directed; they are to consist of two classes, viz. Associates under Patronage, whether of the King or of the Society; and Honorary Associates; from which latter class the Associates under Patronage will chiefly be elected.

The Class of Associates under Patronage, is to consist of persons of distinguished learning, authors of some creditable work of literature, and men of good moral character ten on the Royal endowment, of whom shall be natives of the United Kingdom, and foreigners; and an unlimited number on the funds of the Society, as soon and in proportion as the amount funded shall be sufficient for the purpose: the whole number both on the Royal endowment, and on the funds of the Society, to be appointed by the Council of the Society.

Authors desirous of becoming Associates, shall send a specification of their works, which being approved by the Council of the Society, they will be eligible to the class of Honorary Associates; to which class no person shall be elected, but on the recommendation of at least three Fellows of the Society.

Every Associate under patronage shall, at his admission, choose some subject or subjects of literature, upon which he will engage to communicate within the year a paper or papers for the Society's Memoirs of Literature; of which memoirs a volume will be published by the Society from time to time.

§ 3. The Honorary Members shall be such persons as are entitled to public respect on account of their literary characters, and are to consist of Professors of literature in the several universities of the United Kingdom; Head Masters of the great schools of Royal foundation, and other great schools; eminent literary men in the United Kingdom; distinguished female writers; and also foreigners celebrated for literary attainments.

Subscriptions and Benefactions.

§ 1. An annual subscription of ten guineas continued for five years, and engaged to be continued at least five years more; or, a benefaction of one hundred guineas, will entitle such subscribers and benefactors to privileges hereafter to be declared, according to the date of their subscription. The same privileges to be extended to all other subscribers or benefactors, when their respective subscriptions or benefactions shall collectively amount to one hundred guineas.

§ 2. Honorary Members may become subscribers or benefactors; and, in order that every member of the Society may have an opportunity of contributing to its support, the Associates, of both classes, will be at liberty to subscribe one guinea per annum. Voluntary subscriptions or benefactions from ladies or other persons, not desirous of becoming members of the Society, shall be received, and a list of such contributors shall be inserted in the Society's memoirs.

From the month of November to July, both inclusive, with the exception of the weeks of Christmas, Easter, and Whitsuntide, it is proposed, that a weekly meeting of the Society shall be held on every Thursday, at two o'clock.

⁂ Subscriptions become due every year on the 29th of January, being the anniversary of the King's accession. Subscribers to the Society are requested to send their subscriptions to Messrs. Hoare, Bankers, Fleet-street; or to Messrs. Hatchard and Son, Booksellers, Piccadilly, where copies of the Society's plan, and prize subjects, may be had gratis. Letters and communications to the Society to be directed to the Secretary, at Messrs. Hatchard's.

Form of Order for Payment of Subscription.

Messrs. 1821.

Please to pay forthwith, and annually, on, or as near as may be to the 29th day of January, until further direction, to the Treasurer of The Royal Society of Literature, at Messrs. Hoare, Fleet-street, the sum of guineas.

XV. *Intelligence and Miscellaneous Articles.*

MANGANESE.

To the Editor.

Newcastle-upon-Tyne, July 3, 1821.

Sir, — It may not be uninteresting to such of your readers as possess estates or manorial rights in districts, the geological features of which are similar to those of our coal formation, to be made acquainted with the discovery of the oxide of manganese in this neighbourhood. Flying reports had long been in circulation of the existence of this mineral at Ousten near Urpeth situated between three and four miles north-west of Chester-le-street in the county of Durham; but it was generally surmised that iron slag, of which large quantities occur by the sides of all the Roman roads in the north of England, had been mistaken for it, for no traces of this metal had previously been detected in any of our numerous mines or quarries. However, about two months since, these reports were verified by some large masses of the black oxide being uncovered by the plough, but whether connected with a vein or a bed is not yet determined. The specimen now before me is black, its fracture conchoidal, and structure cellular; the interstices partly filled with iron ochre. Manganese seems to pervade the newest as well as the oldest rocks. Brongniart mentions it in chalk; the black oxide has been detected in the Orkney Islands, and the gray in the slate mountains of Cumberland. The geological position of this coal formation is above the encrinal, and below the magnesian limestone.

While on the subject of localities of rare minerals, it may not be amiss to mention that diallage (forming a subordinate bed in mica schist) was met with three or four years ago by Dr. Bowie at Craig Cailleich in the Highlands, and at Castle Hill near Keswick, by Mr. Jos. Fryer, who has also noticed veins of beautiful yellow ferruginous quartz in the grauwacke at Langholm, bordering on Scotland. Yours, &c. &c.

N. J. W.

AERONAUTIC ASCENSION OF MR. GREEN IN HONOUR OF HIS MAJESTY'S CORONATION.

[His own Account of his Aerial Voyage.]

The balloon with which I ascended was 31 feet in diameter, as near the size as possible of the one with which Lunardi first made an ascension in England. It was inflated with about 1200 cubic feet of carbonated hydrogen gas, supplied from the main pipes of the original chartered Gas Company, and I am much indebted to the gentlemen of the Committee for their kind assistance during the operation of filling. I had no doubt of being able to ascend with the gas, having, since the period when I first conceived the idea that common gas would answer the purposes of aerostation, made frequent experiments, all of which completely succeeded; nor was my ardour damped when I knew that, even within an hour of my ascension, persons of great experience in aerostation expressed their opinion that I should not be able to ascend.

About five minutes before one o'clock the ropes were divided; and having taken my seat in the car, the balloon rose in a majestic manner, nearly perpendicularly. The almost deafening shouts of the populace, and the roar of cannon that took place when I had ascended a considerable distance from the earth, agitated the balloon. I felt the effect of it most sensibly. The moment the discharge of cannon took place, I knew it was the signal to be given when the Crown was set upon the head of my most gracious Sovereign, and I drew the cork of a bottle of brandy, and, having poured out a full glass, I drank "Health, long life, and a glorious reign to His Majesty." The effect of the air upon the brandy is worthy of notice: when I drew the cork, a report took place, which I attribute to the rarification of the air, similar to that produced by drawing a cork out of a bottle of soda water. When the balloon traveiled at its greatest rapidity, I felt not the least motion; it appeared as if the car in which I sat was stationary, and that the earth was receding from me. The balloon took a north-east direction at first; and on my looking down upon the vast assemblage of persons in Westminster, the delight I felt is out of my power to describe. The view presented one entire living mass of more than a million of human beings. Having ascended as high as I could without throwing out ballast, I determined, as the weather was so fine, to keep in sight as long as possible. I threw out two bags of sand of 10 lbs. weight each, and immediately the balloon rose with astonishing rapidity almost perpendicularly, according to my wish. When the balloon arrived at its utmost altitude, which in my opinion (I could not be certain, in consequence of the oscillation of the quicksilver in the barometer) was about 11,000 feet from the earth, I found that I had entered a current of air, conveying me directly eastward, towards the Nore. The cold was extreme. I put on a cloak which I took up with me, and on looking at my glass I found that it was below 30—two degrees below the freezing point. I was fearful of being carried to sea, and immediately opened the valve; the gas issued in considerable quantities; and I found, by the increase of the size of objects below me to my optics, that I was descending very rapidly. The largest fields, which a few minutes before appeared to be not more than six inches square, increased in size greatly; and I very soon saw the sea and a number of vessels most distinctly. The balloon had a rotatory motion, and turned about four times in a minute.

Still fearing that I should fall into the sea, I opened the valve to its utmost extremity; and having descended so as to be able to recognise small objects distinctly on the earth, with great delight I found that the balloon had entered another current of air, which was conveying me from the sea; I was then travelling north-west. I sat down and ate some sandwiches with a good appetite, and saw the clouds rolling beneath me, apparently on the ground. About 20 minutes before two o'clock I descended in a field belonging to a farmer named Lamkins, which is situated about four miles beyond Barnet, in the parish of South Mims. I was not aware that I had descended so rapidly; before I had time to draw myself up to the hoop, the car struck the earth with great force, and I was thrown out of it on my back; I was nearly stunned from the effects of a blow which I received. I still held the hoop of the balloon; and the grappling iron, which I had thrown out when about a quarter of a mile from the earth, not taking firm hold, I was dragged on my back along the ground a considerable distance. The balloon was eventually secured, with the assistance of a gentleman named Waugh, and conveyed to a place of safety in his park, and I was afterwards most hospitably entertained at his mansion. To him my gratitude is due; and but for his kind exertions, I have no doubt the balloon would have suffered considerable injury from the great crowd of persons that assembled on my descent. I believe, from the best calculation I can make, that I travelled altogether, in various directions, upwards of 50 miles.

49, Goswell-street, July 20. CHARLES GREEN.

REPORT RELATIVE TO THE MOVING BOG OF KILMALEADY, IN THE KING'S COUNTY, MADE BY ORDER OF THE ROYAL DUBLIN SOCIETY.

To BUCKNAL M'CARTHY, Esq. &c. &c.

Royal Dublin Society-House, July 10, 1821

Sir,—In compliance with the request of the Royal Dublin Society, conveyed to me by your letter of the 11th inst. I have visited the moving bog of Kilmaleady; and finding on my return to Dublin to day, that very erroneous notions, respecting its magnitude and destructive effects, have been entertained, I think it my duty immediately to communicate to you, for the information of the Society, some accounts of the nature and extent of this once alarming phænomenon.

The bog of Kilmaleady, from whence the eruption broke out, situated about two miles to the north of the village of Clara, in the King's County, is of considerable extent; it may probably contain about 590 acres; in many parts it is 40 feet in depth; and it is considered to be the wettest bog in the county. It is bounded on all sides, except the south, by steep ridges of high land, which are composed, at the top, of lime-stone, gravel, and beneath of cavernous limestone-rock, containing subterraneous streams; but the southern face of the bog is open to a moory valley, about a quarter of a mile in breadth, which for nearly half a mile in length takes a southern direction in the lands of Lesanisky, and then turns at right angles to the west, and continues gradually widening for upwards of two miles. Throughout the centre of this valley flows a stream about 12 feet in breadth, which serves as a discharge for the waters from the bog and surrounding country, and finally joins the river Brusna, above the bridge of Ballycumber.

The bog of Kilmaleady, like all other deep and wet bogs, is composed, for the first eight or ten feet from the surface downward, of a reddish brown spongy mass, formed of the still undecomposed fibres of the bog moss (*Sphagnum palustre*) which by capillary attraction absorbs water in great quantity. Beneath this fibrous mass, the bog gradually becomes pulpy, till, at length, towards the bottom, it assumes the appearance, and, when examined, the consistence of a black mud, rather heavier than water.

The surface of the bog of Kilmaleady was elevated upwards of 20 feet above the level of the valley, from which it rose at a steep angle; and its external face, owing to the uncommon dryness of the season, being much firmer than usual, the inhabitants of the vicinity were enabled to sink their turf holes, and cut turf at a depth of at least 10 feet beneath the surface of the valley, and, in fact, until they reached the blue clay which forms the substratum of the bog. Thus the faces of many of the turf banks reached the unusual height of 30 feet perpendicular; when at length, on the 19th day of June, the lower pulpy and muddy part of the bog, which possesses little cohesion, being unable to resist the great pressure of water from behind, gave way, and, being once set in motion, floated the upper part of the bog, and continued to move with astonishing velocity along the valley to the southward, forcing before it not only the clamps of turf on the edge of the bog, but even patches of the moory meadows, to the depth of several feet, the grassy surface of which heaved and turned over almost like the waves of the ocean; so that in a very short space of time the whole valley, for the breadth of almost a quarter of a mile between the bog edge and the base of the hill of Lisanisky, was covered with bog to a depth of from eight to ten feet, and appeared every where studded with green patches of moory meadow.

The hill of Lisanisky retarded the progress of the bog for some time; but at length it began to flow at right angles to its first course along the valley, where it turned to the west, and continued with unabated rapidity until it reached the bog road of Kilbride, (which runs directly across the valley, and is elevated five or six feet above it,) and choked up by the bridge through which the waters of the stream pass. This barrier retarded the progress of the bog for five days; at the end of that time, the accumulation was such from the still moving bog and the waters of the stream, that it flowed over the road, and covered the valley to the south of it for about half a mile, flowing with varied velocity, till it was again stopped for a few hours (as I understand) by a second road across the valley leading from Clara to Woodfield: having also overcome this obstacle, it proceeded slowly westward; and if its progress had not been checked by the very judicious means that have been employed, the whole extent of the valuable meadows, which compose the valley where it expands to the westward, must long since have been covered. But when the flowing bog had passed over the road of Kilbride, and the consternation in the country became general, at the desire of the lords justices, Mr. Gregory employed Mr. Killaly, engineer to the directors general of inland navigation, to carry into execution any works that could be devised to arrest the progress of the bog. Mr. Killaly at once perceived that the only feasible remedy was to draw off the water that had accumulated; and to accomplish this end, he employed a number of labourers, to open the course of the stream where it was choked up, and also the drains through the valley that could be directed into the stream. By this means the head of the water was soon lowered, and in consequence the bog ceased to flow, and all the loose masses which floated on the river were broken to pieces by labourers placed at intervals throughout its course.

Such was the situation of affairs on my arrival at the bog early on Saturday morning. During the course of the day, I exerted myself to carry into execution the well advised plans which had previously been commenced by Mr. Killaly. Towards evening, the floating masses which came down the river began to lessen considerably both in size and number; and finding every thing proceeded with regularity and certainty, I thought it useless to remain longer.

At present I entertain no apprehension of further devastation from the bog, except in the event of a very great fall of rain during the present week. Slight rains would be of service to increase the current of water, and facilitate the removal of a considerable deposit of heavy, black, bog mud, which at present fills the bottom of the stream. The general current has, however, been much increased, by the breaking down of the weirs on the river Brusna, below the junction of the bog river.

I shall now describe the present appearance and state of the bog and moory valley.

In the centre of the bog, for the space of about a mile and a half in length, and a quarter of a mile in breadth, a valley has been formed, sloping at the bottom from the original surface of the bog, to the depth of 30 feet, where the eruption first took place. In this valley or gulf there are numberless concentric cuts, or fissures, filled with water nearly to the top.

The valley, between the edge of the bog and the road of Kilbride, for the length of half-a-mile, and an extent of between 60 and 80 acres, may be considered as totally destroyed. It is covered by tolerably firm bog, from six to ten feet in depth, consisting, at the surface, of numberless green islands, composed of detached parts of the moory meadows, and of small rounded patches of the original heathy surface of the bog, varying from two to ten feet above its former course, so as to flow over the road.

Beyond the road to Kilbride the bog has flowed for one mile westward, and covered from 50 to 70 acres; in this part the heathy patches of bog gradually lessen in quantity; the green islands disappear, and nothing is observed but a thin deposit, consisting of granulated black bog-mud, varying from one to three feet in thickness. This, though destructive for the present year, may when dry be burnt, and removed for manure to the neighbouring uplands, or left on the spot to fertilize the valley.

Thus the whole distance which the bog has flowed is about three miles in length, namely, one mile and a half in the bog, and the same distance over the moory valley; and the extent covered amounts to about 150 acres.

I have the honour to be, Sir, your most obedient humble servant, RICHARD GRIFFITH, mining engineer.

THE NORTHERN EXPEDITION.

We have been favoured with the following interesting extract of a letter from one of the gentlemen employed on the northern expedition:

"His Majesty's ship Fury, Hudson's Bay, the
Coast of America, June 26, 1821.

"I take the opportunity of writing you, by the return of the Nautilus transport, which accompanied us to carry our heavy stores. We have had an excellent passage from the Orkneys to this part of the world; the weather, however, since we have been here, has not been so favourable.

"We have made two attempts to unload the transport, having made fast to icebergs for that purpose, but have been blown off successively by heavy gales, with the loss of some of our boats from the deck, and no small share of tribulation for the transport, which has not been properly fortified for the ice. She has come off, however, very well, considering everything, having only lost the copper from her bows. We are now taking advantage of a fine day, and hope to get rid of her in a day or two, and to proceed on our destination.

"We made an island about a week ago, called Resolution Island, where we expected to see some Indians; but there was so much ice between the ships and the land, that we could not get in.

"I can hardly give you an idea of our intended route, or, more properly, of our ideal route; first, because our course must, in a great measure, depend upon the state of the ice; secondly, for want of a chart; for those in common use are so incorrect in the general outline of the coast, as to be perfectly useless. If, however, you should fall in with a good map of the country, I will tell you the track we shall endeavour to take.

"After making Cape Farewell, the southern extremity of Greenland, in lat. 59. N. and long. 44. W. we proceeded nearly due west between Cape Chidley, on the Labrador Coast, and Resolution Island, in lat. 61. 40 N. and long. 63. W. where we now are; from hence we intend to steer, if wind and ice will permit, about a north-west course, and endeavour to explore an inlet to the east of Repulse Bay, which has never yet been entered by any one but Fox, about 150 years ago; thence we shall proceed to Hearne's Sea, where we shall winter (if we get there); then to Mackenzie's Sea, Behring's Straits, &c.

"All the officers are exceedingly agreeable, and I have but little doubt we shall spend the winter very comfortably together. We are all preparing our rifles for shooting deer, with which these islands abound. We are, however, exceedingly well off in the eating way—plenty of fresh beef, mutton, pork, eggs, fish, and poultry on board, besides sheep, pigs, and 22 fine bullocks, on board the transport, and potted meats and soups of all kinds for more than three years, so that our salt provisions we scarcely need taste the whole voyage, unless we choose.

"The mean temperature where we now are is about 35° Fahrenheit, the sun just skimming below the horizon at this time at midnight, so that we have constant day, which you may conceive is a great comfort in navigation amongst ice. An apparatus was yesterday let down to the depth of 500 fathoms, for bringing up water: its temperature by a registering thermometer was 40¼ degrees Fahrenheit; that at the surface being 36 degrees. The specific gravity, at the same depth, was 1.0278, and at the surface, 1.0260. Our position, as determined astronomically, is always to the north-west of our dead reckoning; from which it appears, that there is a *constant* current setting from the north-west to south-east."

LIST OF PATENTS FOR NEW INVENTIONS.

To William Church, of Threadneedle-street, for improved apparatus for printing.—Dated 3d July 1821.—6 months allowed to enrol specification.

To James Simpson, of the Strand, surgical-instrument maker, for improvement in the manufacture of snuffers.—3d July.—2 months.

To William Coles, of New-street-square, mechanic, for braces or instruments for the relief of hernia or ruptures.—5th July.—6 months.

To Charles Newman, of Brighton, coach-master, for improvement in the construction in the body and carriage of a stage or other coach, by placing a certain proportion of the outside passengers in the centre of the carriage, and a proportion of the luggage under the same, producing thereby safety to the coach and convenience to the passengers.—17th July.—2 months.

To Dr. Tilloch.

Gosport Observatory, June 11, 1821.

SIR,—Agreeably to the solicitation of John Farey, Esq., Sen., in your May Number [see this also page 22], I inclose my observations on the State of the Barometer, &c. at the proposed hours. Mine is a wheel barometer, manufactured by a good workman in London. I find it sensibly affected in its oscillations by very small variations in the weight of the atmosphere; and as it appears, by comparison with two others of a different construction, to be correct, these observations will be found the more valuable, particularly from the certainty of the real height of its basin above low-water mark, namely, 50 feet, and the proximity of its situation to the sea, the highest tides being nearly level with the ground-floor of my house. The attached Thermometer is correctly graduated to Fahrenheit's scale: and the detached is a horizontal day and night self-registering Thermometer, which was also made in London. I am, sir, your very obedient servant,

WILLIAM BURNEY.

Hour		Ther		Sr	Winds	State of the Weather.
1821. June 11th	Inches	°	°	°		
	29 608	53	51	55	N E	Black *cumulostrati*, with white fringed-like portions of cloud mosculated in front, floating from N E, by means of an upper current, and nascent *cumuli* beneath. These clouds were interspersed over the deep blue sky, excepting two or three small spaces to the south of the sun
9	29 655	54	54	53	N N E	The same modifications of cloud increased in density, and inclined to let fall rain
10	29 640	56	58	50	N E	The aspect of the weather nearly the same, but more sunshine through the apertures of the passing clouds.
11	29 640	56	58	50	N N E	A sprinkling of rain at a ¼ before 11 o'clock, which lasted only three minutes. At 11 the clouds assumed the same appearances as at 10
12	29 678	57	59	48	N.N E	A bed of *cirrus* to the N E: a few drops of rain from a passing *cumulostratus*, and only three small spaces of the sky to be seen

Dr. BURNEY's *Observations for July.*

To Dr. Tilloch.

July 9, 1821.

In sending you the observations on the state of the barometer, &c. on the other side, I think it necessary to remark that in these, as well as in the observations sent last month, the height of the Barometer was reduced to the temperature of 32°.

WILLIAM BURNEY.

Hour.	Barom	Ther. Att.	Ther. Det.	Hyg.	Wind.	State of the Weather.
1821. A.M July 9th, 8h	Inches 30 162	° 58	° 60	° 54	N.N.W	Sunshine, with a brisk wind, and linear *cirri* above an extensive bed of *cirrocumulus*, floating in the direction of the wind.
9	30 165	60	62	50	N.W.	The same modifications as above, with the addition of nascent *cumuli* around the horizon—and breezy at intervals
10	30 161	65	67	40	W.N.W	Lofty *cumuli* apparently osculating with the *cirrocumulus*, which latter has changed [illegible] to a watery colour, more particularly the part not in the immediate vicinity of the sun. Light [illegible]
11	30 159	67	69	38	W.N.W	Calm, and overcast with dark *cumulostratus*, except two or three small openings, which showed that the sky had altered its colour from a light to a dark blue; and that a slight condensation of the superior modifications of cloud was going on, was proved by the sinking of the Barometer and Thermometer, and the receding of the index of De Luc's whalebone hygrometer.
12	30 148	66	[illegible]	40	W.N.W	The sky nearly as at 11, but since that hour two very black nimbiferous clouds have passed over towards the S.S.E., with only a few drops of rain, scarcely perceptible on the leads of the Observatory.

Pocklington, Yorkshire, July 11, 1821

Sir,—I again trouble you with some more meteorological observations made at this place, on Monday the 9th of this month.

I am, sir, yours truly,

William Rogerson, jun.

Clock	Barom	Thermom in doors	Thermom out doors	Wind	Weather
8h	29 950	64 0	59 0	N.W.	[illegible] and cloudy, mild and pleasant
9	29·97[illegible]	[illegible]	59 8	W. by N.	[illegible] broken dense clouds [illegible]
10	29 97[illegible]	[illegible]	60 2	"	[illegible]
11	29 98[illegible]	[illegible]	61 5	"	[illegible] thin wh. clouds, gentle breeze [illegible]
12	29 9[illegible]	[illegible]	[illegible]	"	[illegible] broken dense clouds, gentle breeze [illegible]

Lead[illegible]

London, July 20, 1821.

SIR,—I leave for your Magazine the observations made at Leighton, on the 9th instant, as follows:

1821.	Barom.	Ther att	Ther det	Wind	Deno	Weather.
8h	29·904	55	57	N.W.	fine.	
9	29·899	55	58½	N W.	do.	
10	29 899	55½	61	W.	fine.	
11	29·899	56	61	N.W.	cloudy.	
12	29·896	56½	62	N.W.	do.	
1	29·894	57½	62	N.W.	do.	

At Bushey, by Col. BEAUFOY.

1821	Barom	Ther att	Ther det	Wind	Denom	Weather
8h	29·633	53·3	55 5	N.W by W.	fresh.	Fine.
9	29 635	53 6	58	W.N.W.	do.	Do
10	29 632	54·4	60	W.N.W.	do.	Do.
11	29·633	55·	61	W.N.W.	very fr.	Do.
12	29·631	56·5	62	N.W.	do.	Cloudy.
1	29 629	56·2	63	N.W.	fresh.	Do.

Yours in haste,

B. B.

(*From a Correspondent.*)

Died on the 13th of February, at his house in Lower Thornhaugh-street, Bedford-square, after a lingering illness from anasarca, Thomas Cusac, Esq. universally lamented.

The scholar, the patriot, friend and gentleman were in him eminently united. His researches into the most abstruse branches of science were deep, particularly into the nature of comets, an account of which his disconsolate friends may one day present to the world: of this we are the more desirous, as his doctrine is said not only to be entirely new, but to exhibit the greatest share of probability and reason of any system yet proposed. He has left interesting tracts on the history of Britain and Ireland some centuries before Christ, in which the important and long-disputed question, Whether a federal union of the three countries then existed; or if one was considered as paramount over the rest at the period above mentioned? is impartially examined. The investigation seems to indicate his intention of writing the history of both islands prior to the epoch of Alexander, and must prove a most valuable acquisition to the future historian.

To Dr. Tilloch.

Gosport Observatory, July 5, 1821.

SIR,—I herewith forward for your Philosophical Magazine and Journal, a description of a meteoric phænomenon that appeared here last evening, in order to obtain, if possible, a more satisfactory account of it from some other observer situated further to the westward, in which direction it lay.

I find the latitude of this place (which you asked for on the cover of your last Number), from many observations, to be 50° 47′ 38″ north; and longitude 1° 6′ 40″ west of Greenwich. In time 4′ 26″·7.

I am, sir, your obedient servant,
WILLIAM BURNEY.

A meteoric appearance of triangular and spheroidal forms, was observed here last evening (July 4th) between 9 and 10 o'clock, about W. by S. 11° or 12° above the horizon, and to the north of the moon, which was hid by a *cumulostratus*, so as only to show small portions of her deep red crescent at intervals, through the apertures of that compound cloud. Had the moon been some days older, to have enabled her to reflect a strong light in the attenuated haze in which this phænomenon was apparently situated, I should have attributed it to a *paraselene*; as it was not far beyond the ordinary distance of one from the moon, and displayed bright prismatic colours, as deep red, yellow, &c. But the aforesaid forms which it alternately assumed, and which were serrated round the edges; the diverging pencil rays issuing from the object, both in horizontal and perpendicular directions; the surprising contractions and expansions which it repeatedly underwent, from upwards of a degree and a half to a mere point, and then gradually increasing to its former brilliancy and extent; and the changing of its colours, were occurrences which led me to determine that it was not formed by reflection of the lunar rays, but by an electrical light in that part of the haze which was of a cirrostrative quality. About 10 o'clock the above-mentioned cloud, advancing slowly by a freshening breeze, came up and gradually obscured this interesting phænomenon, which had been very conspicuous in a variety of forms and colours for more than half an hour, to the gratification of many who saw it. Some attributed it to the moon distorted; and some to a greatly diffused comet; while others, of a more liberal opinion, thought it was produced by some uncommon light in the haze near the horizon, it having once or twice thrown out vivid coruscations not unlike those of the *aurora borealis*.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon.	Thermometer.	Barometer	State of the Weather and Modification of the Clouds
	DAYS			
June 15	full	64·	30·05	Fine
16	16	57·	30·03	Cloudy
17	17	52·5	30·15	Ditto
18	18	61·	30·06	Fine
19	19	61·	30·	Ditto
20	20	58·	29·87	Cloudy
21	21	60·	29·87	Fine
22	22	55·	29·97	Cloudy
23	23	55·5	30·	Ditto
24	24	56·	29 90	Ditto
25	25	58·	30·	Ditto
26	26	58·5	29·95	Ditto
27	27	60·	29·85	Fine
28	28	63·	29·90	Ditto
29	new	68·	29·80	Ditto
30	1	69·	29·50	Ditto—heavy rain P.M.
July 1	2	55·	29·35	Rain
2	3	63·	29·60	Fine
3	4	60·	29·65	Ditto
4	5	61·	29·83	Cloudy
5	6	61·	29 85	Ditto
6	7	62·	29·60	Ditto—rain P.M.
7	8	53·5	29·66	Ditto
8	9	57·	29·85	Rain
9	10	60·	29·75	Cloudy
10	11	67·5	29·70	Fine
11	12	62·	29·86	Ditto
12	13	62·	29·82	Ditto
13	14	71·	29·60	Ditto
14	15	66·	29·42	Cloudy

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night	Height of the Barom. Inches.	Weather.
June 27	52	66	50	30·15	Cloudy
28	54	65	58	·25	Fair
29	55	72	60	·19	Fair
30	56	74	66	·03	Fair, fall of rain at night.
July 1	57	72	50	29·67	Rain
	50	53	50	·87	Rain
3	50	59	50	·90	Cloudy
4	51	60	55	30·17	Cloudy
5	55	66	56	·25	Fair
6	56	60	54	·01	Showery
7	54	57	50	29 90	Showery
8	52	59	52	30·13	Cloudy
9	59	68	56	·16	Fair
10	57	68	57	·19	Fair
11	52	67	57	·21	Hazy
12	56	62	52	·13	Fair
13	54	65	56	·01	Hazy
14	56	67	57	29·89	Cloudy
15	56	60	55	·74	Rain
16	56	71	60	30·11	Fair
17	60	69	60	·31	Cloudy
18	60	74	60	·36	Fair
19	58	71	64	·08	Fair
20	62	72	59	29·92	Cloudy
21	63	72	60	·84	Showery
22	60	71	61	·61	Fair
23	62	69	59	·72	Stormy
24	60	68	60	·84	Showery
25	60	68	57	·85	Showery
26	60	67	59	·99	Showery

N B The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the
9th July 9 o'Clock M Barom. 30 161 Ther attached 57° Detached 59
— — 10 — — — 30 161 — — — 58 — — 62
— — 1 — N. — 30 166

Erratum.—In Dr. Burney's reply to Mr. Farey's Queries, *for* Mr. Forster, *read* Dr. Thomas Forster.

XVI. *On the Problem in Nautical Astronomy for finding the Latitude by Means of two Observations of the Sun's Altitude and the Time elapsed between them.* By JAMES IVORY, *A.M. F.R.S.*

THE method generally practised in the British Navy for solving this problem was invented by Douwes, an examiner of sea officers and pilots at Amsterdam, who proposed it to the English Admiralty in 1740. It is however no more than a very limited solution; since it can only be applied with the desired success to correct the latitude by account when one of the observations is very near the meridian, or when the middle time is very little different from half the interval between the two observations. In all other cases one application of the rules will hardly lead to a result sufficiently near the truth; and a series of approximations obtained by repeated operations generally converges so slowly that the method is of little practical utility unless it be assisted by some other artifice.

Dr. Brinkley of Dublin, so long ago as 1791, gave a method of correcting the result found by one operation of Douwes's rules, which, unless in particular circumstances, is abundantly exact for nautical purposes. The same astronomer has since reconsidered the subject; and, in the Nautical Almanac 1822, has reduced his method to easy formulæ comprehending every case that can occur in practice. If this improved method be liable to objection, it is on account of the length and embarrassment of the calculation.

Delambre in his Astronomy (vol. iii. chap. 26) has examined the different methods that have been proposed for solving this problem with his usual industry and accuracy. After a careful examination of the different processes with respect to the length of the calculation and the exactness of the result, he inclines to reject all the indirect methods, and to give the preference to the direct and rigorous solution obtained by the rules of spherical trigonometry. Two elaborate articles in the *Conn. des Tems,* 1817 and 1822, written by the same astronomer, are intended to add strength to his opinion. In these he particularly examines the effect produced by supposing, as is usually done, that the sun's declination is equal to the mean quantity between the two observations and suffers no variation in the elapsed time; and he shows that the error arising from this source may be equal to the sun's change of declination. The error may no doubt be obviated by allowing for the variation of this element of the calculation *; but a new rule is required for this purpose, which adds to the length and perplexity of the operations, before too complicated. In the Quarterly Journal of Science, No. 22, Dr. Brinkley has computed an example making allowance for the change of declination; and if a fair comparison be instituted between his calculation and that by the direct method, as applied to the same example in No. 21 of the same Journal, it appears hardly possible to avoid giving the preference in every respect to the latter.

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* Delambre's Astronomy, vol. iii. chap 26. § 110.

I have now to propose a direct solution of the problem, simpler and easier in the calculation than that recommended by Delambre, and which, I conceive, will be found more convenient in practice than the indirect process commonly used. In explaining this solution I shall first assume that the sun's declination is the same at both the observations and equal to the mean quantity between the two times; and I shall afterwards point out an easy way of correcting the error which this assumption introduces in the result.

The principles of the method are contained in this preliminary proposition.

Lemma. (See figure 3, Plate II.) Let the base AB of a spherical triangle AZB, be bisected in O, and through O draw a great circle perpendicular to AB; then having let fall upon this circle the perpendicular ZD from the vertex of the triangle, we shall have these two formulæ, viz.

$$\text{Sin } ZD = \frac{\cos ZB - \cos ZA}{2 \sin AO},$$

$$\text{Cos } DO = \frac{\text{Cos } ZB + \cos ZA}{2 \cos AO \cos ZD}.$$

Conceive a great circle to pass through the points Z and O: then $\cos ZOB = -\cos ZOA = \sin ZOD$. Now, from the two spherical triangles ZOB and ZOA, we get,

$$\text{Cos } ZB = \cos BO \cos ZO + \sin BO \sin ZO \sin ZOD,$$
$$\text{Cos } ZA = \cos AO \cos ZO - \sin AO \sin ZO \sin ZOD;$$

wherefore, by subtracting,

$$\text{Cos } ZB - \cos ZA = 2 \sin AO \sin ZO \sin ZOD;$$

but, in the right-angled triangle ZOD, $\sin ZD = \sin ZO \sin ZOD$; consequently,

$$\text{Cos } ZB - \cos ZA = 2 \sin AO \sin ZD,$$

from which the first of the two formulæ is derived.

Again, by adding the same two equations, we obtain,

$$\text{Cos } ZB + \cos ZA = 2 \cos AO \cos ZO;$$

but, in the triangle ZOD, $\cos ZO = \cos ZD \times \cos OD$; wherefore,

$$\text{Cos } ZB + \cos ZA = 2 \cos AO \cos ZD \cos DO,$$

from which the second formula is deduced.

Now let P be the elevated pole; P A and P B the horary circles passing through the sun's centre at the two observations; then, if each of the arcs P A and P B be equal to the polar distance of the sun, or to the complement of his declination at the middle of the elapsed time, A and B will represent the two apparent places of the sun. Conceive two small circles described upon the surface of the sphere about the poles A and B, and at distances from them respectively equal to the complements of the observed altitudes; these circles will intersect in two points Z and Z′ situated at equal distances on opposite sides of the great circle passing through A and B, and, generally speaking, either of the points Z or Z′ may represent the place of observation. The problem therefore admits of two solutions, the latitude sought being the complement of either of the polar distances Z P or Z′ P.

Draw the great circle P D O D′ to bisect the vertical angle of the isosceles triangle A P B; which circle will therefore intersect the base A B at right angles and will bisect it. Draw the arcs Z D and Z′ D perpendicular to the circle P O; these arcs will be equal, because Z and Z′ are similarly situated with regard to both the circles A B and P O. Then the angle A P B is known, for it is equal to the elapsed time converted into degrees at the rate of 15° to 1^h; wherefore in the right-angled triangle A P O, the hypothenuse A P and the angle A P O, equal to half A P B, being known, the sides A O and O P may be found by the rules of spherical trigonometry. In the triangle A Z B, the two sides Z B and Z A, being the complements of the observed altitudes, are known; and as the arc A O, half A B, has been found, we may compute the arcs Z D and D O by the premised lemma. Now P D is the difference, and P D′ the sum, of the arcs P O and O D; and hence the two sides about the right angle are known in each of the triangles Z P D, Z′ P D′: wherefore we may find the polar distances Z P and Z′ P, and likewise the angles Z P O and Z′ P O, which are the horary angles at the middle time, and the problem will be completely solved.

Although the problem is ambiguous in theory, yet, in most cases, it becomes determinate in practice. In the first place there is no ambiguity when the arc Z′ P is equal to, or greater than, 90°: for the distance Z P between the place of observation and the elevated pole is always less than a quadrant. In order to find a criterion for determining this point without actually computing both latitudes, it is to be observed that the angle contained between the circles Z O and O P is always less than a right angle; and, because in right-angled spherical triangles the sides are of the same affection with the angles opposite to them, it follows that the arc Z D will be less than 90°. Wherefore, Z′ D being less than 90°, the polar distance Z′ P will be greater than, or equal to, a quadrant, according as the known arc PD′, or PO + OD, is greater than, or equal to, a quadrant; in all which cases there is only one solution, by means of the triangle ZPD having the side PD equal to the difference of PO and OD. But when the arc PD′, or DO + OP, is less than 90°, the same pole will be elevated above the horizons of both the zeniths, and recourse must be had to other considerations to distinguish the true solution from the false one. Now, in this case, the zenith Z′ will be always between the great circle AOB and the equator, having a latitude less than the complement of the arc PO: wherefore, if it be known that the latitude of the place of observation is greater than the complement of PO, the ambiguity will be removed, and the true solution will be obtained by means of the triangle ZPD as in the former cases. On the other hand, if the latitude of the ship be less than the complement of PO, both latitudes must be computed; if they be on different sides of the complement of PO, the case will be determined; but if they be both less than that arc, the solution will remain ambiguous unless the latitude by account be known so nearly as to enable the calculator to make a choice. That both the latitudes may be less than the complement of PO, which is the greatest distance between the great circle passing through A and B and the equator, will be obvious if it be considered that the two zeniths may be as near the great circle AB as we please, and may even coincide in one point in its circumference. This ambiguous case can happen but rarely; and when it does occur, the problem will have no pretension to much precision; because the difference between the arcs ZB and ZA, will be so nearly equal to the arc AB, that very small errors in the observed altitudes will occasion a great variation in the position of the points Z and Z′. By means of these observations the ambiguity of the solution is mostly, but not entirely, taken away.

I shall now reduce the foregoing solution into algebraic formulæ of calculation, which will be shorter than giving a rule in words at length. Let h and h' denote the two altitudes, the letter without the accent standing for the greater; D the sun's declination at the mean time between the two observations; and t the angle found by converting half the elapsed time into degrees at the rate of 15° to 1^h: these are the data of the problem. Put also b for half the base, and p for the perpendicular, of the isosceles triangle APB; y for the arc ZD; x for the arc DO; and further, for the sake of abridging, let

$$A = \frac{\sin h + \sin h'}{2},$$

$$B = \frac{\sin h}{2}$$

Then, if λ be the latitude, and S the horary angle of the middle time, which are the things sought, we shall obtain the following formulæ, by means of the premised lemma and the rules for solving right-angled spherical triangles.

$$1.\ \text{Sin}\ b = \cos D \sin t,$$

$$2.\ \text{Cos}\ p = \frac{\sin D}{\cos b},$$

$$3.\ \text{Sin}\ y = \frac{B}{\sin b},$$

$$4.\ \text{Cos}\ x = \frac{A}{\cos y \cos b},$$

$$5.\ \text{Sin}\ \lambda = \cos y \cos (p \mp x),$$

$$6.\ \text{Sin}\ S = \frac{\sin y}{\cos \lambda}.$$

For the sake of illustration, I shall now subjoin some examples; and I have purposely taken them from Dr. Brinkley's Addition to the Nautical Almanac 1822, in order that the two modes of calculation may be more easily compared.

Example 1.

$$\left.\begin{array}{l}\text{Alt. } 21^\circ\ 26' \text{ A.M.}\\ \text{Alt. } 60^\circ\ 56' \text{ A.M.}\end{array}\right\}\ \begin{array}{l}\text{interval, } 3^h\\ t = 22^\circ\ 30'\end{array}\ \left\{\ \odot\text{'s decl. } 1^\circ \text{ N.}\right.$$

$\text{Sin}\ h = 87106$ (1)
$\text{Sin}\ h' = 36542$ (2)
2A, 123948
2B, 50864
A, 61974
B, 25432

Cos D, 9·99993 (3)
Sin t, 9·58284 (4)
Sin b, 9·58277 (5)
$b = 22°\ 29'\cdot8$

Cos b, 9·96563 (6)
A.C 10·03437
Sin D, 8·24186 (7)
Cos p, 8·27623 (8)
$p = 88°\ 55'$

A.C Sin b, 10·41723
Log B, 9·40538 (9)
Sin y, 9·82261 (10)
$y = 41°\ 39'\cdot4$

Cos b, 9·96563
Cos y, 9·87340 (11)
9·83903
A.C. 10·16097
Log A, 9·79221 (12)
Cos x, 9·95318 (13)

Cos y, 9·87340
Cos $(p-x)$, 9·66021 (14)
Sin λ, 9·53361 (15)
$\lambda = 19°\ 58'\ 7$, latitude.

$x = 26°\ 7'\cdot8$
$p - x = 62\ 47\ \cdot2$

Sin y, 9·82261
Sec λ, 10·02696 (16)
Sin S 9·84957 (17)

$S = 45^\circ\ 0'\cdot7$
$t = 22\ \ 30$

$S + t$, $67^\circ\ 30\cdot7$ } Horary angles at the two
$S - t$, $22\ \ 30\cdot7$ } observations.

Here there is no ambiguity, since $p + x$ is greater than 90°.

The exact latitude is 19° 58′ 45″, although the example may have been originally framed by taking it equal to 20°. This is Dr. Brinkley's 2d example (p. 10), who brings out 19° 59′ by one operation of Douwes's rules and the correction by his own method. By the process here followed, to find the latitude requires taking out fifteen numbers from the Tables. Now one operation of Douwes's rules requires taking out twelve numbers, and the correction must double this labour: perhaps it does more, if we consider the length of the calculation, and the embarrassment of having to use different formulæ. Delambre's method requires nineteen different logarithms, besides employing additions and subtractions of the arcs not wanted here.

Example II.

Alt. 76° 6′ A.M. } interval 6h 20′ } ☉'s decl. 20° N.
Alt. 8° 3′ P.M. } $t = 47^\circ\ 30'$ } Lat. by account 9° N.

Sin h = 97072
Sin h' = 14004
2 A, 111076
2 B, 83068
A, 55538
B, 41534

Cos D, 9·97299
Sin t, 9·86763
Sin b, 9·84062
$b = 43^\circ\ 51'\cdot2$

A.C. sin b, 10·15938
Log B, 9·61840
Sin y, 9·77778
$y = 36^\circ\ 50'$

Cos y, 9·90330
Cos $p+x$, 9·33608
Sin λ, 9·23938
$\lambda = 9^\circ\ 59'\cdot5$

Cos b, 9·85800
A.C. 10·14200
Sin D, 9·53405
Cos p, 9·67605
$p = 61^\circ\ 41'\cdot2$

Cos y, 9·90330
Cos b, 9·85800
9·76130
A.C. 10·23870
Log A, 9·74459
Cos x, 9·98329
$x = 15^\circ\ 47'\cdot5$
$p+x = 77\ \ 28'\cdot7$

Sin y, 9·77778
Sec^t. λ, 10·00664
Sin S, 9·78442
S = 37° 30′
t, 47 30
S + t, 85° }
t − S, 10° } Horary angles.

This is Dr. Brinkley's 1st example, p. 9. The exact latitude is 10°, and he brings out 10° 1′ by the same process as in the last example. This instance admits of two solutions, the arc $p + x$ being less than 90°: but the one near the equator is taken, because the latitude by account is set down 9°. The ambiguity will be removed if the other latitude be computed by the formula, $\cos \lambda = \cos y \cos (p - x)$; it comes out 33° 51′.

Example III.

Alt. 70° 1′ } interval 2^h 20′ }
35 21 } t = 17° 30′ } ⊙'s decl. 5° 30′.

Sin h = 93979
Sin h' = 57857
2 A, 151836
2 B, 36122
A, 75918
B, 18061

Cos D, 9·99800
Sin t, 9·47814
Sin b, 9·47614
b = 17° 25′

A.C. sin b, 10·52386,
Log B, 9·25674
Sin y, 9·78060
y = 37° 6′·8

Cos b = 9·97962
A.C. 10·02038
Sin D, 8·98157
Cos p, 9·00195
p = 84° 14′·1
Cos b, 9·97962
Cos y, 9·90170
9·88132
A.C. 10·11868
Log A, 9·88034
Cos x, 9·99902
x = 3° 51′
$p-x$, = 80 23·1

Cos y, 9·90170
Cos $(p-x)$, 9·22279
Sin λ, 9·12449
λ = 7° 39′·2

Sin y, 9·78060
Sec^t λ, 10·00388
Sin S, 9·78448
S = 37° 30′
t, 17 30
S + t, 55° }
S − t, 20 } Horary angles.

This is Dr. Brinkley's third example (pp. 11 and 12). It is an unfavourable instance for his rules, requiring several computations and corrections to arrive at a right result. It admits of two solutions but without ambiguity, if the latitude by account be sufficient to ascertain that the true latitude is less than 5° 46′, the complement of p. The other latitude is 1° 31′·6.

These examples will be sufficient for showing the method of calculation. I proceed now to consider the correction required for the sun's change of declination in the interval between the observations. The true place of the pole will now be at P′, without the great circle DO which bisects the arc AB, because the polar distances P′A and P′B are unequal. Draw P′P perpendicular to that great circle, and complete the isosceles triangle APB. The arcs AP and PB make equal angles with the circle P′P; and hence in the small change of place from P to P′, one of the two arcs AP′ and BP′ will increase just as much as the other decreases; and each of the arcs AP and PB will be equal to half the sum of the polar distances P′A and P′B. We shall therefore obtain the arc ZP by the method already explained; and, having drawn Pm perpendicular to ZP′, the correction we are seeking is $m\mathrm{P}' = \mathrm{PP}' \times \sin \mathrm{P}'\mathrm{P}m = \mathrm{PP}' \times \sin \mathrm{ZPO} = \mathrm{PP}' \times \sin \mathrm{S}$. Also, by the lemma,

$$\mathrm{PP}' = \frac{\cos \mathrm{P}'\mathrm{A} - \cos \mathrm{P}'\mathrm{B}}{2 \sin \mathrm{AO}}.$$

Now, d being the declination at B the greater altitude, and D the mean declination as before, we have

$$\mathrm{P}'\mathrm{A} = \mathrm{PA} - (\mathrm{D} - d),$$
$$\mathrm{P}'\mathrm{B} = \mathrm{PA} + (\mathrm{D} - d);$$

And hence, $\mathrm{PP}' = (\mathrm{D} - d) \times \frac{\sin \mathrm{PA}}{\sin \mathrm{AO}} = \frac{\mathrm{D} - d}{\sin t}$.

Wherefore,

$$\mathrm{P}m = (\mathrm{D} - d) \times \frac{\sin \mathrm{S}}{\sin t}.$$

The corrected latitude will therefore be

$$\lambda - (\mathrm{D} - d) \times \frac{\sin \mathrm{S}}{\sin t};$$

or, independently of S; because $\sin \mathrm{S} = \frac{\sin y}{\cos \lambda}$;

$$\lambda - (\mathrm{D} - d) \times \frac{\sin y}{\sin t \cos \lambda}.$$

Again the arcs P′A and P′B may be considered as making equal angles with PP′: consequently the horary circle at the middle time, which bisects the angle AP′B, will be perpendicular to PP′. Hence the true horary angle of the middle time is equal to the complement of ZP′P. But from the triangle ZPP′, we get

Sin

$$\text{Sin } ZP'P = \sin ZPP'' \times \frac{\sin ZP}{\sin ZP''} = \cos S \times \frac{\cos \lambda}{\cos (\lambda - \delta\lambda)};$$

putting $\delta\lambda$ for mP', the variation of λ. wherefore, if $S + \delta S$ be the true horary angle of the middle time, we shall get

$$\text{Cos } (S + \delta S) = \cos S \times \frac{\cos \lambda}{\cos (\lambda - \delta\lambda)} = \frac{\cos S}{1 + \delta\lambda \tan \lambda}$$

and hence

$$\delta S = \delta\lambda \times \frac{\cos S \tan \lambda}{\sin S} = (D - d) \times \frac{\cos S \tan \lambda}{\sin t}.$$

The corrected horary angle is therefore

$$S + (D - d) \times \frac{\cos S \tan \lambda}{\sin t}$$

By means of these easy formulæ the change of the sun's declination may be allowed for, when this is thought necessary, without hurting the uniformity of the general calculation.

As an example, I shall take the instance in the Quarterly Journal, No. 22, p. 372.

Example IV.

1st Alt. 12° 4'·1 } interval 3h { ☉'s declination 8° 15'
2d Alt. 16 5'·8 } t = 22° 30' } change in 3h, + 3

d = 8° 15'
D = 8° 16'·5

Sin h = 67217
Sin h' = 27726

2 A, 94943
2 B, 39491

A, 47471·5
B, 19745·5

Cos D, 9·99545
Sin t, 9·58284
Sin b, 9·57829
b = 22° 15'·2

A.C. sin b, 10·42171
Log. B, 9·29547
Sin y, 9·71718
y = 31° 25'·6

Cos. y, 9·93112
Cos $p - x$, 9·94594
Sin λ, 9·87706
λ = 48° 53'·4
Sin y, 9 71718
Sec^t λ, 10·18210
Sin S, 9·89928
S = 52° 28'

Cos b, 9·96639
A.C. 10 03361
Sin D, 9·15813
Cos. p, 9·19174
p = 81° 3'·2
Cos y, 9·93112
Cos b, 9·96639
9·89751
A.C. 10·10249
Log A, 9·67643
Cos x, 9·77892
x = 53° 3'·2
$p - x$ = 28 0

In calculating the corrections of λ and S three places of the logarithms are sufficient.

$$D - d = 1'5$$

Log. 1′·5,	0·176	Log. 1′·5	0·176
Sin S,	9·899	Cos S,	9·785
A.C. sin *t*,	10·417	Tan λ,	10·059
Log 3·1,	0·492	A.C. sin *t*,	10·417
		Log 2·7	0·437

λ = 48° 53′·3	S = 52° 28′
− 3′·1	+ 2′·7
48 50′·2	52 30′·7
true latitude.	true hor. angle of M. T.

The method that has been explained requires only the easy lemma for computing the arcs ZD and DO, and the rules for solving right-angled spherical triangles; and it is an advantage that every step is the calculation of some part of the figure, by which circumstance the memory is assisted. The process here followed is also preferable to the other methods in leading to the determination of the problem, or in pointing out which of the two possible solutions is the true one, when this can be done. In the extensive Nautical Tables published by the late Mr. Mendoza, there is one for assisting the direct solution of this problem. It contains the base, and likewise the angle at the base, of the isosceles triangle A P B formed by the two circles of declination. A similar table that should contain the perpendicular P O of the same triangle, and likewise half the base A O, or rather the sine and co-sine of A O, would render the preceding method by far the shortest of any hitherto proposed. But the use of such tables is not free from objection, and ought not to be adopted unless a great advantage is gained.

August 6, 1821. J. Ivory.

XVII. *On the aeriform Compounds of Charcoal and Hydrogen; with an Account of some additional Experiments on the Gases from Oil and from Coal. By* Wm. Henry, *M.D. F.R.S.*

[From the Transactions of the Royal Society.]

The experiments on the aeriform compounds of charcoal and hydrogen, described in the following pages, are supplementary to a Memoir on the same class of bodies, which the Royal Society did me the honour to insert in their Transactions for 1808[*], as well as to other papers on the same subject, which have been published in Mr. Nicholson's Journal, and in the Memoirs of the Manchester Society. Of these essays, I beg leave to offer a very brief recapitulation, with the view merely of connecting them with what is to follow.

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[*] See Phil. Mag. vol. xxxii. p. 277.

In the first of these essays (Nicholson's Journal, 8vo, June, 1805), I detailed a series of experiments on the gases obtained by the destructive distillation of wood, peat, pit-coal, oil, wax, &c., from which it appeared that the fitness of those gases for artificial illumination was greater, as they required for combustion a greater proportional volume of oxygen; and that the gases generated from different inflammable bodies, or from the same inflammable substance under different circumstances, are not so many distinct species, which under such a view of the subject would be almost infinite in number, but are mixtures of a few well known gases, chiefly of carburetted hydrogen with variable proportions of olefiant, simple hydrogen, sulphuretted hydrogen, carbonic acid, carbonic oxide, and azotic gases; and that the elastic fluids obtained from coal, oil, &c. have probably, in addition to these, an inflammable vapour diffused through them when recent, which is not removed by passing them through water*. In the same paper I explained certain anomalies that appear in the experiments of the late Mr. Cruickshank, of Woolwich, which are not at all chargeable as errors upon that excellent chemist, and could only be elucidated by further investigation of the gases to which they relate. Of his labours it would be unjust, indeed, to speak in any terms but those of approbation, for they may fairly be considered as the foundation of most that is now known respecting this species of aeriform bodies. To Mr. Dalton, also, we are indebted for an accurate acquaintance with carburetted hydrogen gas, and for much information that is valuable in assisting us to judge of the composition of mixed combustible gases, by the phænomena and results of firing them with oxygen†.

In the second Memoir (Philosophical Transactions, 1808), I described a series of experiments on the gases obtained from several different varieties of pit-coal, and from the same kind of coal under different circumstances. Various species of that mineral were found to yield aeriform products, differing greatly in specific gravity, combustibility, and illuminating power; the cannel coal of Wigan, in Lancashire, being best adapted to the purpose, and the stone-coal of South Wales the least so. In decomposing any one species of coal, the gaseous fluids were ascertained not to be of uniform quality throughout the process, but to vary greatly at different stages; the heavier and more combustible gases coming over first, and the lighter and less combustible afterwards. By subsequent experiments on the gases obtained from coal on the large scale of manufacture, it was found that a similar decline in the value of the products takes place, but not to the same extent, owing, probably, to the greater uniformity of temperature which is attainable in large operations *.

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* Nicholson's Journal, 8vo. xi. 72.

† New System of Chemical Philosophy, *passim*.

On the practical conclusions, which it was the object of the last mentioned Essay to establish, I forbear to dwell, because they are unconnected with my present purpose, which is limited to the chemical constitution of these compound gases, and to the methods of separating them accurately from each other. The view of their nature and composition, which was taken in the first Essay, was opposed by those able philosophers M. Berthollet, and Dr. Murray, of Edinburgh, who both contended for greater latitude as to the proportions in which hydrogen and charcoal are capable of uniting, and considered these proportions indeed as subject to no limitation. The facts, however, which have since been multiplied in this, as well as in other departments of chemistry, tending to prove that bodies capable of energetic combination unite in a few definite proportions only, leave little doubt that the same law holds good with respect to the compounds of hydrogen and charcoal Not that it is meant that the known compounds of those elements are the only possible ones; for others will probably be discovered, which will still be found conformable to the general law, *that when one body combines with another in different proportions, the greater proportions are multiples of the less by an entire number*.

A different view of the subject has lately been taken by the ingenious author of the Bakerian Lecture, published in the Philosophical Transaction for 1820. In that paper, Mr. Brande has endeavoured to prove, that the gas called light carburetted hydrogen, or simply carburetted hydrogen, or hydro-carburet, is not entitled to be considered as a distinct species; that the only aëriform compound of charcoal and hydrogen, which is with certainty known to exist, is the gas called olefiant, or bi-carburetted hydrogen; and that the gases evolved by heat from coal and oil, are in fact nothing more than mixtures of olefiant and simple hydrogen gases in various proportions.

In assuming, in the first Essay, the existence of light carburetted hydrogen as a definite compound, characterized by its requiring, for the complete combustion of each volume, two volumes of oxygen, and giving one volume of carbonic acid, I relied on the sole authority of Mr. Dalton; for the gas of marshes, though before known to be inflammable, had not been subjected to accurate examination by any other chemist. Mr. Cruickshank, indeed, speaks of it as "pure hydro-carbonate †;" but since he classes it in that respect with the gas obtained by the destructive distillation of camphor, from which it differs essentially in composition, it is plain that he was not correctly acquainted with the properties of pure carburetted hydrogen. Previously to the second set of experiments, I satisfied myself by the careful analysis of a specimen of the gas from stagnant water, for which I was indebted to Mr. Dalton, that it really has the properties which have been ascribed to it by him as characteristic; and in 1807 I found precisely the same characters in the fire-damp of coal-mines *. Dr. Thomson, also, from experiments in 1811 †, on the gas from stagnant water, and Sir Humphry Davy ‡, from the analysis of the fire-damp in 1815, drew the same conclusions. It is in the power, indeed, of every chemist to investigate for himself the properties and composition of carburetted hydrogen gas, since it may easily be procured in considerable quantity, by stirring the bottom of almost any stagnant pool, especially if composed of clay. During the last summer, I obtained it from a source of this kind, which afforded it in such abundance, that several gallons might have been collected in a few minutes. This gas I submitted to repeated and most careful examination. It contained $\frac{1}{20}$th its volume of carbonic acid, but no sulphuretted hydrogen whatever, and no proportion of oxygen gas that could be discovered by attentively testing it with nitrous gas. The results of its combustion with oxygen gas, effected in a Volta's eudiometer in the usual manner, showed that it was contaminated with $\frac{1}{15}$th its volume of azotic gas. Apart, however, from this, the pure portion, in a great number of trials, required, as nearly as can be expected in experiments of this sort, two volumes of oxygen for combustion, and gave one volume of carbonic acid. Its specific gravity, taken on quantities procured at three several times, varied only from ·582 to ·586, the mean of which is ·584; and thus, allowing for $\frac{1}{15}$th of azotic gas of specific gravity ·972, gives ·556 for the specific gravity of pure carburetted hydrogen gas, a number which coincides almost exactly with that found by Dr. Thomson §. Since, therefore, the same results have been obtained from the examination of gases similarly collected at distant times and places, there appears to me no reason for refusing to consider carburetted hydrogen gas as a true chemical compound, characterized by perfect uniformity of properties and composition. At the temperature of 60° Fahrenheit, and under 30 inches pressure, 100 cubical inches must weigh 16·95 grains, and be composed (taking the weight of 100 cubic inches of carbonic

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* Manchester Society's Memoirs, New Series, vol iii.

† Nicholson's Journal, 4to. vol. v. p. 6

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[*] Nicholson's Journal, 8vo. xix. 149.

[†] Mem. of the Wernerian Society, i. 506.

[‡] Phil. Trans. 1816, p. 9.

[§] Annals of Philosophy, vol. xvi. p. 252.

acid at 46·5 grains, and the charcoal in 100 grains of that acid at 27·3 grains) of

	Grains.	Grains.	Grains.
Charcoal	12·69	74·87	100
Hydrogen	4·26	25·13	33·41
	16·95	100.	133·41

And olefiant gas (giving twice its volume of oxygen by combustion, and weighing 29·64 grains for 100 cubical inches*) must be constituted of

	Grains.	Grains.	Grains.
Charcoal	25·38	85·63	100
Hydrogen	4·26	14·37	16·71
	29·64	100·	116·71

And as 16·7 is to 100, so very nearly is 1 to 6, which last number is the weight of the atom of charcoal, as deduced from the constitution of olefiant gas. It is true, that this determination a little exceeds that which is derived from the composition of carbonic acid (viz. 5·65), the atom of oxygen being taken at 7·5. But if 8 be the true number for oxygen, which now seems to be most probable both from experiment and analogy, we shall then find an exact coincidence between the relative weight of the atom of charcoal, as deduced from olefiant gas, and as determined from carbonic acid. Perhaps the true specific gravity of hydrogen gas, on which depend the relative weights of the atoms of hydrogen and oxygen, may be fully as correctly ascertained from the composition of carburetted hydrogen, as by direct attempts to weigh so light a fluid. Now, as the hydrogen in 100 cubic inches of hydro-carburet weighs only 4·26 grains, and is equivalent to 200 cubic inches of hydrogen gas, we have 2·13 grains for the weight of 100 cubic inches of hydrogen gas, from which may be deduced ·0698 for its specific gravity, that of air being 1. And if the specific gravity of oxygen gas be 1·111, it will be found that the two volumes of hydrogen, required to saturate one volume of oxygen gas, have as nearly as possible the relative weight of 1 to 8.

Were any additional argument necessary to establish the existence of carburetted hydrogen as a distinct species, it might be derived from the action of water on that gas, which, besides being absorbable in a constant proportion, admits of being expelled again by the application of heat, not otherwise changed than in having acquired a small quantity of those gases which are always present in water, and of which it is impossible to deprive it even by long continued boiling.

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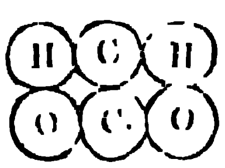
* I adopt this result of Dr Thomson from its near coincidence with that of an experiment of my own, on the specific gravity of olefiant gas, published in the Phil Trans 1808, p 293

The process, by which carburetted hydrogen gas is evolved in natural operations, is no doubt the decomposition of water, and admits of being explained on the atomic theory of Mr. Dalton, by supposing two atoms of charcoal to act at once on two atoms of water. One atom of charcoal attracts the two atoms of hydrogen, forming carburetted hydrogen gas, and the other atom of charcoal unites with two atoms of oxygen, constituting carbonic acid. This is illustrated by the annexed figure, in which two atoms of charcoal C.C. are represented as interposed between two atoms of water, each consisting of an atom of hydrogen and an atom of oxygen. Dividing the diagram vertically into three parts, we have the original substances; and separating it horizontally, we obtain the two new compounds. This theoretical view of the subject is confirmed by the fact, that the carburetted hydrogen, formed at the bottom of stagnant pools, is never accompanied by carbonic oxide, but always by carbonic acid, the full quantity of which is prevented from appearing, in consequence of the absorption of a great part of it by the mass of water, under which the changes are taking place.



Being provided with such an abundant supply of carburetted hydrogen, I availed myself of it to examine the mutual action of that gas and chlorine on each other, principally with a view to ascertain, how far reliance may be placed on the latter as an instrument in the analysis of mixed combustible gases. This is a part of the subject that was first investigated, though with a different view, by Mr. Cruickshank[*]. He observed that a mixture of chlorine with hydrogen, carburetted hydrogen, or carbonic oxide, in certain proportions, kept in a bottle entirely filled with the mixture, and furnished with an air-tight stopper, did not exhibit any immediate action, but that in twenty-four hours, on withdrawing the stopper, the fluid immediately rushed in, and filled most of the space originally occupied by the gases. But he was not aware of the influence of light on these changes, which was discovered about the same time by Gay Lussac[†] and by Dalton[‡]. It does not, however, appear to have been ascertained by either of them, whether the complete exclusion of light prevents any degree of action of chlorine and carburetted hydrogen on each other. I mixed, therefore, those two gases in different proportions in well stopped vials, which were completely filled with the mixture, and covered by opake cases. When the stoppers were removed under water, at various intervals after the mixture, from a few minutes to 39 days, no diminution whatever of volume was found to have taken place; and after having removed the chlorine by liquid potash, the carburetted hydrogen gas gave the usual products of carbonic acid, and consumed the usual proportion of oxygen. Mixtures also of hydrogen and chlorine, and of carburetted hydrogen and chlorine, standing over water in graduated tubes, which were shaded by opake covers, sustained no loss of bulk, except what arose from the absorption of chlorine by the water, the combustible gas remaining wholly unaltered. It may be considered, therefore, as quite essential to the mutual agency of these gases, that they should be subjected to the influence of light. But it is not necessary that the direct rays of the sun should fall on the mixture, the light of a dull and cloudy day being fully adequate to the effect. On a day of this sort, I filled several stoppered vials, graduated into hundredths of a cubic inch, with a mixture of 30 volumes of carburetted hydrogen with from 80 to 90 of chlorine, and uncovering them all at the same moment, exposed them to the feeble light which was then abroad. By exposure of one of the vials during half a minute, no diminution of volume was found to have been effected; another vial, opened under water when one minute had elapsed, showed an absorption of five parts; a third in two minutes had lost fifteen parts; a fourth in four minutes 25 parts; and a fifth, opened in five minutes, contained only 50 volumes out of the original 110.

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[*] Nicholson's Journal, 4to. v. 202.

[†] *Mem. de la Soc. d'Arcueil*, ii 349.

[‡] New System of Chemical Philosophy, p 300.

The products, resulting from the contact of carburetted hydrogen and chlorine, under circumstances favourable to their mutual action, have been described by Mr. Cruickshank, with whose experience on this point my own entirely agrees. When rather more than four volumes of chlorine are kept in mixture with one volume of gas from stagnant water, the products are muriatic acid gas, and a volume of carbonic acid equivalent to that of the pure carburetted hydrogen; and this, whether the mixture be exposed to direct or indirect solar light; the only difference being that the less intense the light, the more slowly is the effect produced. When less than four volumes of chlorine are employed, the residue consists of muriatic and carbonic acids, carbonic oxide, and undecomposed carburetted hydrogen, the proportions of the two last increasing as, within certain limits, we reduce the relative quantity of chlorine. These changes were ascertained, both by Dr. Davy and the late Dr. Murray, to depend on the presence of moisture, which is unavoidably introduced in the common mode of operating; for when the gases, first perfectly dried, were mixed in an exhausted glass vessel, and exposed even to the direct rays of the sun, no mutual action was found to ensue. In the theory of these changes there is, it must be confessed, a little uncertainty. Does the chlorine, it may be asked, act simultaneously on the hydrogen of water, and on that of the combustible gas; or does it decompose water only? The former view of the subject appears to me most probable, because, if the chlorine acted on water only, free hydrogen would be evolved from that portion of the hydro-carburet which abandons its charcoal to the oxygen of the water; which is not consistent with experience. When it is required to form carbonic acid, four volumes of chlorine must be used for the decomposition of each volume of carburetted hydrogen. In this case, two atoms of chlorine unite with the two atoms of hydrogen existing in the combustible gas, and the two other atoms of chlorine with the two atoms of hydrogen from the water. But to convert carburetted hydrogen into carbonic oxide, three atoms of chlorine are sufficient, two of which are employed as in the first case, and the third is expended in saturating the hydrogen of one atom of water, which supplies to the charcoal an atom of oxygen for the formation of carbonic oxide. Calculating in the same manner, we shall find, also, that three atoms of chlorine are adequate to convert one atom of carbonic oxide into carbonic acid.

The facts which have been stated sufficiently prove, that chlorine cannot be employed as a means of correctly analysing mixtures of olefiant gas, either with hydrogen or with carburetted hydrogen, if light be admitted, even though of feeble intensity, and for the short interval during which such an experiment may be expected to continue: and they explain that uncertainty as to the results of analyses of mixed gases made in this way, which was first remarked by Mr. Farraday*, and subsequently by myself†. Chlorine becomes, however, a most useful agent in separating olefiant gas from such mixtures, provided light be entirely excluded during its operation, as I have found by subjecting to its action mixtures of those gases with known proportions of olefiant gas. In these analytical experiments, I admitted into a graduated tube standing over water, a volume of chlorine exceeding by about one half what was known to be sufficient, and noted its bulk when actually in the tube, which was immediately shaded by an opake cover. A measured quantity of the mixture was then passed up, and in about ten minutes the outer cover was cautiously lifted, till the surface of the water appeared. The diminution of volume thus ascertained, divided by 2, was found to give pretty correctly the quantity of olefiant gas known to be contained in the mixture. But the greatest precision was attained by waiting fifteen or twenty minutes, and then quickly washing the remaining gas with dilute solution of potash, in order to remove the excess of chlorine. From the volume of the residuary gas, it was necessary to deduct the amount of impurity previously ascertained to exist in the chlorine; and the remainder, taken from the volume of mixed gases which had been operated on, showed how much olefiant gas had been condensed by the chlorine. When very narrow tubes were employed, and the column of gases mixed with chlorine was of considerable length, a longer continuance of the experiment was found necessary, and the gases were suffered to remain in contact during an hour or more. In this way it was ascertained, that olefiant gas may be accurately separated by chlorine from hydrogen, carburetted hydrogen, or carbonic oxide gases, or from mixtures of two or more of those gases, which are left quite unchanged in volume and in chemical properties, when light has been carefully excluded from the mixture.

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* Journal of Science, &c. vi. 358.

† Manchester Memoirs, New Series, vol. iii.

This property of chlorine is the foundation of a fresh analysis, to which I have thought it expedient to submit the gases from coal and oil, in order to decide what aëriform fluids remain after the separation of that portion which is condensible by chlorine; —whether the residue consists, as I have heretofore maintained, of carburetted hydrogen chiefly, with variable proportions of hydrogen and carbonic oxide; or whether, according to the new view of the subject, it consists of hydrogen gas only.

In the experiments made for this purpose, I operated generally on from 60 to 80 cubic inches of oil gas or coal gas, assaying a small specimen first, as a guide to the quantity of chlorine which it would be necessary to employ. The volume of chlorine thus found to be requisite, and about half as much more, was passed into an air receiver standing over water, and completely shaded by an opake cover which was fitted over it. The oil or coal gas was then added by degrees, if much condensation was expected, because in that case a considerable increase of temperature would have been produced by the sudden admixture of large quantities; or at once, if only a moderate action had been indicated by the previous assay. The mixture was allowed to stand, completely guarded from the light, during 30 or 40 minutes, or even longer, and the residue was expeditiously washed with liquid potash, and a small portion again assayed, to ascertain that the action of the chlorine was complete. The specific gravity of the washed gas was then carefully taken, that of the entire gas having been previously determined: and the results of its combustion with oxygen examined, and compared with those of the gas in its original state.

[The Continuation of this Paper, containing Experiments on the Gas from Oil and from Coal, in our next.]

XVIII. *On*

XVIII. *On the Discovery of a North-west magnetic Pole. By Colonel* MACDONALD*.

Summerland-place, Exeter, July 12.

MUCH useful discussion has arisen in consequence of the dissertations on the interesting science of *Magnetism* and *Variation*, inserted in your Numbers of December and January†: and in all instances, the reasoning and suggestions alluded to have experienced the marked approbation of characters eminent for their knowledge of a subject rendered extremely prominent by the recent brilliant discovery of a *North west Magnetic Pole*.

The above-mentioned papers on magnetic variation having been published previously to the appearance of the valuable works of Captain Parry and of Mr. Fisher, some further thoughts necessarily arise from a due consideration of statements and opinions therein contained; and such remarks as are offered are made with the best of views, viz. that of calling the attention of men who have equally the power and inclination to promote objects of public utility.

Voyages of discovery, and travels, are nationally undertaken on three principles, at once creditable, legitimate, and laudable. On the first, the Deity is honoured by the humble but hazardous efforts of his creatures, to discover the extent of his wonderful works here on earth, and the nature of uneducated man under the varying aspect of climate and seasons: and that too with the noble ultimate view of ameliorating his condition, by conferring the benefits of knowledge, and the blessings of Religion. On the second principle, the discoveries of enterprising mariners and travellers can alone (as in the present instance) enable us to advance certain sciences which require experiments of a delicate description to be made, and observations of an accurate nature to be taken, in opposite and unfrequented paths of the world. The third principle, sanctioning distant research by sea and land, or that of forwarding the interest of commerce and arts, may not be less recommendable; as thereby civilization and the comforts of life are materially benefited, and human happiness consequently increased.

If the two voyages of discovery in search of a North-west passage into the North Pacific, or Eastern Ocean, should not attain that object, they will prove of incalculable value in ultimately establishing, on sure and fixed scientific principles, the wonderful rule, or rationale of the variation of the Magnetic Needle; provided we avail ourselves skilfully of the means furnished by the daring and so far successful enterprise of men of consummate

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* From the Gentleman's Magazine for July 1821.

† See Phil. Mag. for February 1821.

courage and perseverance, amidst appalling difficulties, and trials almost superhuman.

Though currents and other circumstances sufficiently evince the existence of a North-west passage, it would appear, from the accounts before us, that there cannot be a hope of accomplishing it in the parallel of the newly-discovered Georgian Islands. In your Number of January, it was recommended to attempt to effect a passage into the Hyperborean Sea, out of Repulse Bay, at the North extremity of Hudson's Bay; and there, at this moment, the discovery ships are making such attempt. This dreary and inhospitable coast runs nearly East and West, about the parallel of 70°, and between 90° and 160° of West longitude, to Icy Cape, where the American coast runs South-south-west to Behring's Straits. We have no accounts of this coast on which any reliance can be put; and if we credit such as we have, the sea in these Northern regions is constantly frozen up. It appears from Cook's Voyages, that even in summer the sea was frozen over between the Russian and American coasts. This shows, that whatever may be the result of the present attempt to the East or West of Southampton Island, there cannot remain the slightest hope of effecting the passage through Behring's Straits. In former statements, there was some reason to suppose that the passage would be achieved through the Polar Basin, considerably to the Northward of the parallel of the new discoveries, with the disadvantage of a longer run than by the usual course. If, however, the northwest passage can be made along the North coast of America, as now attempting, certainly, the run to India, and especially to China, will be shorter; but in such case, the risk, hazard, and danger would be constantly imminent. Ships so situated would be liable to be crushed to pieces by ice-bergs; would be frequently rendered immoveable by sudden or continued congelations of the ice; would at a certain time of the year be enveloped in darkness; or would always have the greater part of their crews disabled by intensity of cold, and undergoing the amputation of limbs mortified by the stoppage of the current of life.

If commerce is to derive benefit from any new or additional productions to be yielded by these unexplored Seas, Islands, and Coasts, it is evident that the Hyperborean Coast itself, and not ships, must be the medium of procuring such advantage. It is probable that sledges may travel along the ice on this coast; or at various stations on it, such as Mackenzie's River, or Coppermine River (provided wood is found on, or can be floated down to, the coast), stout small vessels might be constructed for the purpose of proceeding northward among (as yet undiscovered) islands, in favourable seasons. But this is under a supposition that incurred expense would be more than defrayed by commercial returns.

Having premised thus much, I come now to the most important object of this paper, and paramount to every other consideration attached to the subject. If no other advantage arose from the present voyages than the recent discovery of a North-west Magnetic Pole, that alone is so valuable to science in establishing, in process of time, a sure theory of the Magnetic Variation, so indispensable for nautical purposes, that the best thanks of the country are due to the Admiralty for the efficient manner in which these voyages have been directed. In giving such requisite efficacy, the talents, knowledge, and general information of that able and useful character, Mr. Barrow, have been essentially subservient.

When your Number for January was published, it was not distinctly known, that among the Georgian Islands the movement of a balanced needle became so weak and sluggish as to be nearly annihilated; that is to say, the magnetic action of the real North Pole of the Earth became as nothing compared to the strong and direct attraction of the North-west Magnetic Pole, evidently situated within the Earth, and in a site very nearly under the sea-surface moved over by the Discovery-ships. For centuries have ingenious philosophers been conjecturing the existence of one or more Magnetic Poles, in endeavouring to reduce visible effects to causes, and to form theories, if not demonstrable, at least plausible. At length, to the honour of the British nation, the first in arts, arms, and philanthropy, all doubt and uncertainty are happily removed; and by proceeding on scientific principles, through the medium of accurate experiments, the complete establishment of a theory of the Magnetic Variation is now attainable. The continued course of experiments formerly recommended to be made in a situation contiguous to the Magnetic Pole will not be practicable in that situation, on account of a strength of attraction downwards so great there as to turn the needle nearly into a continuation of that Pole, an effect shown to demonstration, by experiments made by means of powerful magnets acting on common needles. It is fortunate that the requisite series of experiments cannot be efficiently made near the site of the newly-discovered Pole, as the intensity of the cold there would render a continuance of life nearly impossible. It is evident that the Discovery-ships crossed a meridian under which this Pole, and the North Pole of the Earth, became in one and the same vertical plane. Here, of course, there would be no variation, as the needle would be acted on by both Poles in a line, or in conjunction with its position. On the parallel of latitude of 60°, such line of no variation must be found by trial made by scientific, persevering, and skilful men, to be employed for this very important purpose. These men must travel westward from

Hudson's Bay, till they, by accurate magnetic observations, find themselves in this requisite situation *. Here, then, a building for their accommodation should be erected; and a smaller one, devoid of iron, must cover a meridian accurately laid off, according to a process described in my papers on this subject, in the Philosophical Transactions. Such an instrument as is used at our Society's rooms must be applied to this meridian, as that is superior in construction to that used by me for similar purposes, on Sumatra, and St. Helena. The primary and direct object in view, is to ascertain by three daily observations, the decrease of variation, under the meridian, in order to arrive ultimately at the law of movement of the North-west Magnetic Pole, either round the Terrestrial Pole, on a parallel of latitude, or otherwise in a straight line, within the earth, and between two points in its parallel of position. This motion will be so slow, as to require a series of years to arrive at the proper scientific conclusions deducible from such requisite experiments. It may be again urged, that such a magnetic movement is compatible with the supposed solidity of the earth. I refer to my former statement on this part of the subject, and such philosophers as are Christians (and the most able have been such) I refer to St. Paul's Epistle to the Ephesians, chap. iv. verse 9. It being highly probable, from close considerations of the variation in South latitude, that the South-east end of the new pole has a corresponding movement round the South pole of the earth, I would strongly recommend that a similar series of experiments be made on the South side of New Shetland, which I conjecture to be a continuation of the Southern Thule, in longitude 30° West, and 60° South latitude. Similar observations ought to be made on the Island of Desolation in latitude 49° South, and longitude 70° East; and also in North latitude, on Spitzbergen and Nova Zembla.

Royal patronage and munificence could not be more nobly applied, than in pursuits so honourable to man, and beneficial to human happiness. Monarchs or men thus occupied, might legitimately say,

> "—— Tentanda via est, quâ me quoque possim
> Tollere humo, victorque virûm (rerum) volitare per ora."

If in time it became ascertained that the N.W. and S.E. magnetic poles had a regular movement round the poles of the globe, the variation and all its anomalies would be accounted for, and other magnetic phænomena, equally surprising and unaccountable, would be reduced to a certain theory.—As things are, we ob-

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* From the supposed position of the Magnetic Pole, it might not be necessary to proceed inland, westward, above five degrees, or 150 miles, about the parallel of 60° North latitude.

serve effects which we cannot trace to any satisfactory cause. I am in habits of collecting facts which may, aided by the observations of others, lead at some future period to legitimate conclusions. I try all bodies of iron by means of a sensitive magnet, and find in them properties not generally understood. I find that a good magnet will equally, as by electricity or galvanism, impart polarity to needles, *by mere juxta-position.* I have rendered magnetic three pieces of wire, situated in a semicircular form, opposite to the poles of a powerful magnet.—All bars standing or fixed perpendicularly (such as all iron railings in streets) are magnetic; the North pole being at the bottom, and the South at the top. The bottoms or lower parts of all common chimney-grates are North, while the tops are South poles. The iron handles of pumps are magnetic; the furthest out-end being a North, while the end nearest to the pump is a South pole. Large weighing weights possess polarity; as also all iron bars for sale in shops. It is a curious fact, that the uppermost part or top of the iron round a carriage-wheel attracts the North end of a magnet, and is consequently a South pole, while the lower part of the same iron in contact with the ground, attracts the South end of the needle, and is therefore a North pole. Turn the same wheel round half a circle, and these poles will immediately become *reversed.*

I mention these few out of many experiments, in order to induce others to assist in ascertaining facts, with a view of establishing what is now wanting,—*a sure Magnetic Theory.*

Yours, &c. JOHN MACDONALD.

XIX. *Answer to "Remarks on Mr.* RIDDLE's *Claim to the Invention of a new Method of determining the Latitude."* *By Mr.* EDWARD RIDDLE.

To Dr. Tilloch.

SIR,—IN answer to the extraordinary charges of your correspondent γ, I beg leave simply to state, That in the 8th volume of the Edinburgh Phil. Trans., which was published in 1818, and which I first saw in October of that year, General Brisbane intimated his intention of making a communication "On the mode of determining latitudes by the sextant most correctly by a series of observations made near noon."—That this was the whole of the announcement, and that it was unaccompanied by any hint respecting the nature of the method, or any other remark whatever.

That in October 1818 I transmitted to you a full account of a method of finding the latitude *by observations made with the sextant near noon*, which I had practised for a considerable time previous; and that the observations in the accompanying example were made on September 24, 1817.

That in the spring of 1821 I observed a notice in the Edinburgh Phil. Journal, that Gen. Brisbane's promised communication on this subject was just published in the Edinburgh Phil. Trans.; and in May 1821, when volume ix. part I. of the Edinburgh Phil. Trans. was received in Newcastle, I saw the paper itself for the first time. It forms article XIV. of the part.

That although it is thus impossible I could have been indebted to General B. for a method which I had previously practised for several years, and had actually *published* two years before I had any means of knowing what his method was, in a work in the hands of every scientific person in Europe; our methods are *not only generally similar*, but *absolutely the same both in principle and in all their practical details.*

That, *whatever Gen. B. may have done*, I have never seen any of the three foreign works in which your correspondent says the substance of the same method is to be found.

That, *if I were disposed to quibble*, I might say that the designation of "a new method" is not mine, as you know very well, sir, that the title of my letter in which that designation is introduced was prefixed by yourself.

That, though I am sure every thing is done at Greenwich in the best possible way. I believe I need not say that the observations made at that admirable establishment are made with other and better instruments than a sextant and an artificial horizon. And, finally, that the charge of incorrectness in an approximate formula arising from substituting the *arc* of *one second* for the *sine* of the same arc, requires no notice.

With respect to the insinuation that I did *not* practise the method of finding the time which I stated myself to have practised, till I saw Gen. B.'s communication on the subject;—the *affirmative*, as the matter stands, depends on my integrity;—the negative rests not on any authority whatever. From *myself*, on *this* subject, no other reply will be expected.

Your obedient servant,

EDWARD RIDDLE.

Trinity House School, Newcastle,
Aug. 6, 1821.

XX. *A Communication of a singular Fact in Natural History. By the Right Honourable the Earl of* MORTON, *F.R.S., in a Letter addressed to the President**.

MY DEAR SIR,—I YESTERDAY had an opportunity of observing a singular fact in natural history, which you may perhaps deem not unworthy of being communicated to the Royal Society.

Some years ago, I was desirous of trying the experiment of domesticating the Quagga, and endeavoured to procure some individuals of that species. I obtained a male; but being disappointed of a female, I tried to breed from the male quagga and a young chesnut mare of seven-eighths Arabian blood, and which had never been bred from: the result was the production of a female hybrid, now five years old, and bearing, both in her form and in her colour, very decided indications of her mixed origin. I subsequently parted with the seven-eighths Arabian mare to Sir Gore Ouseley, who has bred from her by a very fine black Arabian horse. I yesterday morning examined the produce, namely, a two-years old filly, and a year-old colt. They have the character of the Arabian breed as decidedly as can be expected, where fifteen-sixteenths of the blood are Arabian; and they are fine specimens of that breed; but both in their colour, and in the hair of their manes, they have a striking resemblance to the quagga. Their colour is bay, marked more or less like the quagga in a darker tint. Both are distinguished by the dark line along the ridge of the back, the dark stripes across the fore-hand, and the dark bars across the back part of the legs. The stripes across the fore-hand of the colt are confined to the withers, and to the part of the neck next to them; those on the filly cover nearly the whole of the neck, and the back as far as the flanks. The colour of her coat on the neck adjoining to the mane is pale, and approaching to dun, rendering the stripes there more conspicuous than those on the colt. The same pale tint appears in a less degree on the rump; and in this circumstance of the dun tint also she resembles the quagga.

The colt and filly were taken up from grass for my inspection, and, owing to the present state of their coats, I could not ascertain whether they bear any indications of the spots on the rump, the dark pasterns, or the narrow stripes on the forehead, with which the quagga is marked. They have no appearance of the dark line along the belly, or of the white tufts on the sides of the mane. Both their manes are black; that of the filly is short, stiff, and stands upright, and Sir Gore Ouseley's stud-groom alleged that it never was otherwise. That of the colt is long, but so stiff as to arch upwards, and to hang clear of the sides of the neck; in which circumstance it resembles that of the hybrid. This is the more remarkable, as the manes of the Arabian breed hang lank, and closer to the neck than those of most others. The bars across the legs, both of the hybrid and of the colt and filly, are more strongly defined, and darker than those on the legs of the quagga, which are very slightly marked; and though the hybrid has several quagga marks, which the colt and filly have not, yet the most striking, namely, the stripes on the fore-hand, are fewer and less apparent than those on the colt and filly. These circumstances may appear singular; but I think you will agree with me, that they are trifles compared with the extraordinary fact of so many striking features, which do not belong to the dam, being, in two successive instances, communicated through her to the progeny, not only of another sire, who also has them not, but of a sire belonging probably to another species; for such we have very strong reason for supposing the quagga to be.

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* From the Transactions of the Royal Society for 1821, Part I.

I am, my dear sir,
Your faithful humble servant,
Dr. W. H. Wollaston. MORTON.

P. S. I have requested Sir Gore Ouseley to send me some specimens of hair from the manes of the sire, dam, colt, and filly; and I shall write to Scotland for specimens from those of the quagga and of the hybrid.

I am not apt to build hypotheses in a hurry, and have no predilection either for or against the old doctrine of impressions produced by the imagination; but I can hardly suppose that the imagination could pass by the white tufts on the quagga's mane, and attach itself to the coarseness of its hair.

Wimpole-street, Aug 12, 1820.

Note by Dr. Wollaston.

By the kindness of Sir Gore Ouseley, I had an opportunity of seeing the mare, the Arabian horse, the filly, and the colt, and of witnessing how correctly they agreed with the description given of them by Lord Morton.

Having shortly afterwards described the circumstances to my friend Mr. Giles, I found that he had observed some facts of nearly equal interest, of which, at my request, he has since sent me the following account.

XXI. *Particulars of a Fact, nearly imilar to that related by Lord* MORTON, *communicated to the President in a Letter from* DANIEL GILES, *Esq.*

IN answer to your inquiries, I will now give the best account I can of my sow and her produce.

She was one of a well known black and white breed of Mr. Western, the Member for Essex. About ten years since I put her to a boar of the wild breed, and of a deep chesnut colour, which I had just received from Hatfield House, and which was soon afterwards drowned by accident. The pigs produced (which were her first litter) partook in appearance of both boar and sow, but in some the chesnut colour of the boar strongly prevailed.

The sow was afterwards put to a boar of Mr. Western's breed (the wild boar having been long dead). The produce was a litter of pigs, some of which we observed, with much surprise, to be stained and clearly marked with the chesnut colour which had prevailed in the former litter.

This sow had afterwards another litter of pigs by a boar of Mr. Western's breed, and I think, and so does my bailiff, that some of these were also slightly marked with the chesnut colour; but though we noticed the recurrence with surprise, it is so long since, that our recollection is much less perfect than I wish it to be.

I should observe, that I have known Mr. Western's breed many years, but never in any other instance observed the least appear- of the chesnut colour. Believe me, &c.

Youngsbury, Nov. 10, 1820. DANIEL GILES.

XXII. *On the Use of Shot Cartridges.* *By* A CORRESPONDENT *in India.*

To the Editor.

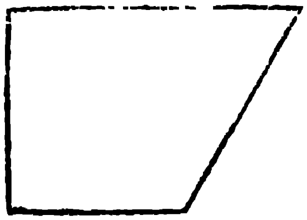
DEAR SIR,—IF you consider the following subject worthy a place in any of the Numbers of the Philosophical Magazine, you are welcome to the communication; it may I think prove useful to sportsmen. I have been in the habit of shooting for some years past, and have always loaded with shot cartridges, the advantage of which I think worth attention. In the first place, using the cartridge ensures the sportsman a more rapid succession of discharges, if the game be numerous. No wadding is required after the first or second discharge. The shot is not wasted by scattering it out of the measure, which is frequently the case when loading in a hurry. The touch-holes are not damaged by the return of shot into the breeching of the gun, which I have frequently observed, and which is probably occasioned by bad powder, the succeeding discharge generally forcing the shot into the touch-hole itself, making it ragged and enlarged. The shot cartridges fired at ten yards or less distance from a sheet of paper, will cover it as well as if blown from a gun loaded in the usual way.—I have frequently tried my own guns, of Joseph Manton's make, with cartridges at a sheet of paper placed twenty or thirty yards distant, the charge being one ounce of number 4 shot; the only comparative difference I ever observed was that a few more shot were thrown within the compass, consequently it may be inferred that my way of loading carries closer. No difficulty attends making the cartridges; the accompanying No. 1 is the exact pattern of the paper, which should be thin and soft. No. 2 is the former upon which the paper is rolled spirally from the broad perpendicular side, and then doubled into the hollow at the end. The cartridges when wanted for use may be carried in a common canvass shot bag.

To conclude: I may mention that this way of loading is very general in India, and I doubt not will be approved of in England if ever tried. I am, dear sir, yours very obediently,

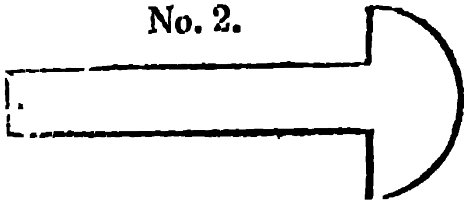
B. C. S.

Province of B.
April 1820.

No. 1.



No. 2.



XXIII. *Report of a Committee of the Academy of Natural Sciences of Philadelphia, on a new Hydrostatic Balance invented by* Isaiah Lukens. *Read May* 26, 1818*.

The undersigned Committee beg leave to report, that the instrument invented by Mr. Lukens, and referred to them by the Academy, consists of a very sensible steelyard or Roman balance, so arranged as to be particularly adapted to the finding of specific gravities. The arms of the balance are so constructed, in the first instance, as to be in exact equipoise, when unloaded. The object [C, fig. 1, Plate II.] of which the specific gravity is to be ascertained is suspended to the shorter arm, by any of the usual methods; and its relative weights in air and in water are indicated by the numbers on the graduated arm [A] at which the moveable weight or pea [D] is suspended, when the beam is brought into a horizontal position. It is evident that the absolute weight of the pea is arbitrary, and it is one of the advantages of the instrument that the pea may be altered to suit the weight of the object under trial; even a stone of a proper size might be employed, and would always be at hand.

When great accuracy is desired, a second pea is employed, which must be either one tenth, or one-hundreth part the weight of the first. The larger pea will then indicate the units of weight, and the smaller the tenths or hundredths. The same object might also be obtained by suspending the pea to the middle of a Vernier-scale.

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* From the Journal of the Academy, Vol 1. Part II.

The instrument, and its necessary appendages, are arranged in a small box, so as to be very convenient, and very portable.

Your Committee, after a due consideration and an actual trial of this apparatus, are of opinion, that, for facility and rapidity of operation, it has the advantage over every other that has hitherto been proposed for the same purpose; and they therefore cheerfully recommend it to the attention of the Academy.

They propose that it should be named Lukens's Hydrostatic Balance.

All which is respectfully submitted.

WILLIAM MACLURE.
R. M. PATTERSON.
ISAAC LEA.

XXIV. *Description of a Hydrostatic Balance, by which the Specific Gravities of Minerals may be ascertained without Calculation. By* BENJ. H. COATES, *M.D. Read June* 16, 1818*.

THE present instrument (see Plate II. fig. 2,) has arisen from one lately presented to the Academy, in which the common steelyard is employed for this purpose.

The object of the alteration is, without rendering the instrument more complicated, or more troublesome in its application, to save the labour and inconvenience of calculation. By means of it, the specific gravity of a mineral may be ascertained in a few moments, and without pen and ink, or any other assistance than a cup of water. With the aid of the neatness and convenience of the instrument on which it is grafted, it is hoped to be a practical saving of time and labour to the mineralogist.

The lever resembles that of a common steelyard, and is contrived to balance exactly, by making the shorter end wider, and with an enlargement at the extremity. The upper edge of each limb is rectilinear, and free from notches, for the sake of accuracy in adjusting the weights.

The shorter end is undivided; but on the longer is inscribed a scale, of which every division, reckoning from the extremity of the lever, is marked with a number, which is the quotient of the length of the whole scale, divided by the distance of the division from the end. Thus, at half the length is marked the number 2, at one-third, 3, at one-fourth, 4, &c. Also at two-thirds the length is marked $1\frac{1}{2}$, at two-fifths, $2\frac{1}{2}$, &c. And so of all the fractions, sufficiently minutely. These numbers extend as high as the specific gravity of platina;—the pivot of the instrument represents unity, and a notch is made at the further end.

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* From the Journal of the Academy of Natural Sciences of Philadelphia, Vol. I. Part II.

In using this instrument, any convenient weight is suspended by a hook from the notch at the end of the scale. The body under examination is to be suspended to the other end by a horse-hair, and slid along till an equilibrium is produced. It is then, without altering its situation on the beam, to be immersed in water, and balanced a second time by sliding the weight. The hook of the latter then marks the specific gravity on the scale.

The demonstration of this is very simple. The instrument being supposed in equilibrium, and B D (see figure) and the weight of the counterpoise being constant, the weight of the body varies as the distance of the counterpoise from B, by the common principle of the lever. Hence, if C be the place of the weight at the conclusion of the operation,

Weight in water : weight in air : : B C : B A. And, by subtraction, the loss of weight in water : weight in air : : A C : A B; and hence

$\frac{\text{wt in air}}{\text{loss}} = \frac{A\,B}{A\,C} =$ the spec. grav.; which is the rule. Q. E. D.

Substances lighter than water may have, if necessary, their specific gravity ascertained by the usual method; a scale of equal parts being cut on the opposite side of the beam, and the article to be weighed placed in a notch for the purpose. For mineralogy, however, this will seldom be necessary. The bottom of the notch A (at the smaller end) should be in a line with the edge of the scale, its sides being a little raised. The top of the shorter end should be rather the thickest part of it, to allow the horse-hair, by which the mineral is suspended, to swing clear. This mode will be found very delicate and accurate, and a hook must not be used, as it cannot be balanced.

The instrument, in this form, is exceedingly compact, and may be reduced to a simple rod.

The principle is capable of being applied (as in an instrument I have made) to an arc of a circle, with a rod resembling in its application a common bent lever.

XXV. *True apparent Right Ascension of Dr.* MASKELYNE's 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. GROOBY.

[Continued from p 52]

Aigu-

1821	γ Pegasi	α Arietis	α Ceti.	Aldebaran	Capella	Rigel	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydrae.	Regulus.	β Leonis.	β Virginis.	Spica Virginis	Arcturus.
	H. M. 0 3	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
Oct.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	5·83	10 45	59 84	43 46	33 58	59·56	3·06	32 40	17 90	13 31	58 43	24 32	49 39	52 04	57 25	23·17	48 16	31 66
2	83	46	86	49	62	59	09	43	93	35	46	35	41	06	26	18	16	65
3	83	48	86	51	66	61	13	46	96	38	49	38	44	08	28	19	16	65
4	84	49	90	54	71	64	16	49	99	41	52	42	46	10	29	21	17	64
5	84	50	92	57	75	67	19	52	18 02	45	55	45	49	12	30	22	17	64
6	84	52	94	60	79	70	23	55	05	48	58	48	51	14	31	23	17	63
7	85	53	96	63	83	72	26	58	08	52	61	52	53	16	33	24	17	63
8	85	55	97	65	87	75	29	61	10	56	64	56	56	18	35	26	18	63
9	85	56	99	68	91	78	33	64	13	60	67	59	59	21	36	28	19	63
10	85	57	53 0 00	71	95	80	36	66	16	63	70	62	61	24	38	29	19	63
11	85	59	02	73	99	83	39	69	19	67	73	65	64	26	40	31	20	63
12	85	60	04	76	34 03	85	42	72	22	71	76	69	66	29	41	33	21	63
13	85	62	05	79	06	88	45	75	25	74	79	72	69	31	43	34	22	64
14	85	63	07	81	10	91	48	78	28	78	82	76	72	34	45	36	23	64
15	85	64	09	84	14	93	52	81	31	82	85	79	74	37	46	38	23	64
16	85	66	10	87	18	96	55	84	34	86	88	82	77	39	48	40	24	64
17	85	67	12	90	22	99	58	87	37	89	92	86	79	42	50	41	25	64
18	85	68	13	92	26	60 01	61	90	40	93	95	90	82	45	52	43	26	64
19	85	69	15	94	30	04	64	92	43	97	98	93	85	48	54	45	27	65
20	85	70	17	96	33	06	67	95	46	14 00	59 01	97	88	50	56	47	28	65
21	84	71	18	99	37	09	70	98	49	04	04	25 01	91	53	58	49	30	65
22	84	72	20	44 01	40	11	73	33 01	52	08	07	04	94	56	60	51	31	66
23	84	72	21	03	44	14	76	03	55	11	10	08	96	59	62	53	32	66
24	84	73	23	06	48	16	80	06	58	15	13	11	99	61	64	55	33	67
25	83	74	24	08	51	19	83	09	61	19	16	15	50 02	64	66	57	34	68
26	83	75	26	10	55	21	86	12	64	23	19	19	05	67	68	59	35	69
27	83	76	27	13	59	24	89	15	67	26	23	22	08	70	70	61	37	70
28	83	76	29	15	62	26	92	17	70	30	25	25	11	73	72	63	38	70
29	83	77	30	17	65	28	95	20	73	34	28	29	14	76	75	66	40	71
30	82	78	31	19	69	30	97	23	76	37	31	33	17	79	77	68	42	72
31	82	79	32	21	72	33	4 01	26	78	41	31	36	20	82	79	71	43	73

1821	1α Libræ	2α Libræ	α Cor Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri	2α Capri	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Oct.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	50 13	1·58	8 36	29 46	28 44	31 61	40 11	54 67	48 01	6·05	34 39	46 83	10·58	22·44	39·11	48 92	54 93	13·30
2	12	56	35	45	43	59	09	65	48 00	03	38	82	57	42	10	92	93	30
3	12	56	33	44	42	57	07	62	47 98	01	36	81	56	39	09	92	92	31
4	11	55	32	43	40	56	06	60	96	6 00	35	79	54	37	09	91	92	31
5	11	55	31	42	39	54	04	58	95	5 98	33	78	53	35	08	91	92	31
6	11	55	30	41	38	53	02	55	93	96	32	76	51	32	07	90	91	31
7	11	55	29	40	37	51	40 01	53	92	95	30	75	50	30	06	89	91	32
8	10	54	28	34	36	49	39 99	50	91	93	28	74	49	27	05	89	91	32
9	10	54	27	39	35	48	98	48	89	92	27	73	48	25	04	88	90	32
10	10	54	26	38	34	47	7	46	87	90	25	71	46	23	03	87	90	32
11	10	54	26	38	33	45	96	44	86	89	24	70	45	21	02	87	90	32
12	10	54	25	37	32	44	93	41	84	87	22	68	43	19	02	86	89	32
13	09	53	24	36	31	43	92	39	83	86	21	67	42	16	01	85	89	32
14	09	53	23	36	31	41	90	36	81	84	19	65	40	14	39 00	84	88	32
15	09	53	23	35	29	40	89	34	80	83	18	64	39	12	38 99	83	87	32
16	09	53	22	35	28	38	87	32	78	81	16	62	37	09	98	82	87	32
17	09	53	21	34	27	37	86	29	77	80	15	61	36	07	97	82	86	32
18	09	53	21	34	26	36	85	27	75	78	13	59	34	04	96	81	86	31
19	09	53	20	34	26	35	84	25	73	77	12	58	33	22 02	95	80	85	31
20	09	53	20	33	25	34	83	23	71	75	10	56	31	21 99	94	79	84	31
21	10	54	19	33	25	32	82	20	70	74	09	55	30	97	93	78	83	30
22	10	54	18	33	24	31	81	18	68	72	07	53	28	95	91	77	82	30
23	10	54	17	32	24	30	79	16	66	70	06	52	27	92	90	76	81	30
24	10	54	17	32	23	29	78	14	64	69	04	50	25	89	89	75	80	29
25	11	55	16	31	22	28	77	11	63	67	03	49	24	87	88	74	79	29
26	11	55	16	31	22	27	76	09	61	65	01	47	22	84	87	73	78	28
27	12	56	15	31	21	26	75	07	60	64	34 00	46	21	82	86	72	77	28
28	13	57	15	31	21	25	74	05	58	62	33 98	44	19	79	85	71	76	27
29	14	58	15	31	21	24	73	03	56	61	97	43	18	77	83	70	75	26
30	14	58	15	31	21	24	72	02	55	59	95	42	17	74	82	69	74	26
31	15	59	15	31	20	23	71	00	53	58	94	40	15	71	81	68	73	25

XXVI. *No-*

XXVI. *Notice respecting a Volcanic Appearance in the Moon, in a Letter, addressed to the President. By Captain* HENRY KATER, *F.R.S.*

London, Feb 8, 1821.

DEAR SIR,— IT may perhaps be interesting to the Royal Society to be informed, that on Sunday evening, the 4th instant, I observed a luminous spot in the dark part of the moon, which I was inclined to ascribe to the eruption of a volcano.

The telescope used was an excellent Newtonian of 6½ inches aperture, with a power of 74. The moon was exactly two days old, and the evening so clear, that I was able to discern the general outlines in the dark part of her disk. Her western azimuth was about 70°, and her altitude about 10 degrees.

In this position at 6 hours 30 minutes, the volcano was situated (estimating by the eye) as in the accompanying sketch [distant from the northern limb of the moon about one-tenth of her diameter]. Its appearance was that of a small nebula subtending an angle of about three or four seconds.

Its brightness was very variable; a luminous point, like a small star of the 6th or 7th magnitude, would suddenly appear in its centre, and as suddenly disappear, and these changes would sometimes take place in the course of a few seconds.

On the evening of the 5th, having an engagement which prevented my observing it myself, I arranged the telescope for two friends, who remarked the same phænomena as the night before, but in an inferior degree, partly perhaps in consequence of the evening not being so favourable.

On the 6th, I again observed it; it had certainly become more faint, and the star-like appearance less frequent. I could see it very distinctly with a power of 40. As the moon approached the horizon, it was visible only at intervals when the star-like appearance took place. On the same evening I had the pleasure of showing it to Mr. Henry Browne, F.R.S.

I regret that I had no micrometer adapted to my telescope; but I have reason to believe the distance of the volcano from the edge of the moon was about one-tenth of her diameter, and the angle it formed this evening with a line joining the cusps was about 50°.

I remarked near the edge of the moon, a well known dark spot, from which the volcano was distant, as nearly as I could estimate, three times its distance from the edge of the moon.

In a map of the moon published by Dr. Kitchener (and which is the best small map with which I am acquainted), there is a mountain sufficiently near the situation of the volcano, to authorize the supposition that they may be identical.

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* From the Transactions of the Royal Society for 1821, Part I.

On the 7th, I could still see the volcano, and the occasional star like appearance; but I do not think it was sufficiently perceptible to have been discovered by a person ignorant of its precise situation. I am inclined however to think, that the difficulty of seeing it is rather to be attributed to the increased light of the moon, than to the diminished action of the volcano.

I have the honour to be, dear sir, &c.

To Sir H. Davy, Bart. P.R.S. &c. HENRY KATER.

P.S. Since the preceding letter was written, I have ascertained that the spot in which I observed the volcanic appearance is that named Aristarchus. This spot was particularly examined by Hevelius, who calls it Mons Porphyrites, and who considers it to be volcanic. If his drawings are to be relied upon, it has undergone a considerable change in its appearance since his time.

Sir William Herschel has recorded in the Philosophical Transactions an observation of three volcanoes, which he perceived in the moon, April 19, 1787, at $10^h\ 36^m$, sidereal time. One of these, which he says showed "an actual eruption of fire or luminous matter," was distant from the northern limb of the moon $3'\ 57''\cdot 3$, the diameter of the burning part being not less than $3''$. I find that this observation was made about 9 o'clock in the evening, when the moon was not quite two days old; and from the situation of the spot described by Sir William Herschel, I have no doubt of its being the same that I have noticed.

XXVII. *The first Portion of a Catalogue of 1800 zodiacal Stars, for the Epoch of January 1, 1800; from the Works of* PIAZZI, BODE, *and others, with illustrative Notes. Selected and arranged by a Member of the Astronomical Society of London.*

In the present and five following portions, it is intended to offer to the amateurs of Astronomy, a Catalogue of Stars lying within 10° on either side of the Ecliptic; arranged in the order of their passing the meridian, and containing not only the mean Right Ascensions and Declinations, but as many other accurate and useful particulars, as the compiler's materials, and the limits of an octavo page, will permit.

By referring to the notice inserted p. 394 of the preceding volume, it will be seen that a *general catalogue* of stars is announced, comprising in number about 4000, and extending over that part of the heavens which is visible to British observers. This extent it was intended to divide into four districts, and the most natural arrangement would be to commence with that which is constantly above our horizon. The slow progress, however, which will be made in the publication, renders it desirable to commence with the more important zodiacal region.

An account of the several existing catalogues having been drawn up by the present writer, and inserted in the last volume of the Phil. Mag., renders it unnecessary to repeat in this place those details, which would otherwise have formed part of the present introduction.

The catalogue consists of twelve columns, the contents of which shall be explained *seriatim*.

1—3. Under the head "Synonyms," are in the first place comprised three columns. The first contains the number by which each star is distinguished in Piazzi's last catalogue. It is essential to notice, that every hour of Right Ascension commences a fresh numerical series; consequently, in quoting from Piazzi, it is necessary to prefix to the number of the star, the hour of Right Ascension as marked in Roman numerals at the top of the seventh column. The second column exhibits in like manner the number by which the star is distinguished in Bode's folio catalogue; with this difference, that here the numbers recommence with each constellation, which makes it necessary to mention the latter, when quoting from Bode. The third column includes the number by which the star is known in the general catalogue of Flamsteed (frequently termed the *Britannic*) or the zodiacal ones of Mayer, or La Caille. Although the same star should be found in more than one of the three last mentioned catalogues, yet it is considered sufficient to give the reference to one only: and the reader should bear in mind, that it is by the denomination given in this third column, that the star is most commonly known among astronomers. The numbers from Mayer or La Caille have the letters M and C prefixed to them respectively; those from Flamsteed are without any letter. It is almost superfluous to observe, that where the third column is blank, the star is not found in either of the three catalogues to which that column is appropriated.

4. contains the Greek or Roman *character*; and includes not only those adopted by Flamsteed (which vary a little from the original ones of Bayer), but also the additional ones first introduced by Bode in his *Uranographia*, which have not before been noticed in any English work. These additional characters are included between parentheses.

5. The name of the *constellation* is inserted in the fifth column, but is abbreviated to save room. A dash signifies the same as the preceding line. The boundaries of constellations being altogether arbitrary, it may be readily supposed, that the catalogues frequently disagree in this particular. The Brit. Cat. is well

known to have been very negligently drawn up in this respect; yet the numbers have acquired by long usage such a degree of authority, as to render any alteration of boundary an invidious task. It has been, however, attempted by Bode, with considerable success. In the present compilation, where a star has been placed by Bode in a constellation different from that assigned by Flamsteed, both constellations are put down, and the number of each catalogue placed in a line with its proper constellation, as may be seen in the instances of 54 Ceti, 16 Trianguli, and others. As to the numerous disagreements between Bode and Piazzi, the same minuteness has not been thought necessary, and in these cases the authority of the former is implicitly adopted. An *obelisk* inserted in this column, indicates that a note is attached to the star.

6. The *magnitude* of each star is given almost invariably from the same authority as its mean position.

7. The approximate *Right Ascension in Time.* Here the *minutes* only of right ascension are inserted in the column, and the *hours* stand at the head of it. If greater accuracy be wished for, it may be derived from the quantity in the next following column, by the well known rule for converting space into time.

8. The mean *Right Ascension in degrees,* minutes, seconds, and tenths, is in general taken from the accurate catalogue of Piazzi. A few stars are inserted, which do not occur in that work, and with regard to these, the authorities for position will be found in the notes.

9. The *Annual Variation* in right ascension, comprises the joint effect of *precession* and *proper motion,* which the limits of the page would not permit to be given separately. Should the precession alone be wished for, it may be readily had from the formulæ:

$$\text{An. Pre. in R.A.} = 46''\cdot011 \pm 20''.046 \sin \text{R.A.} \tan \text{Decl.}$$

$$\text{————— Decl.} = 20''\cdot016 \cos \text{R.A.}$$

The above coefficients are Bessel's, those used by Piazzi are 46″·0395 and 20″·064. In every case where the star is found in Bessel's Catalogue, the precession as well as proper motion is taken from it; in other cases Piazzi's numbers for the right ascension are diminished by 0″·04 between 64″·16 and 53″·40, by 0″·03 thence to 42″·65, and by 0″·02 thence to 31″·90. The error occasioned by this approximate method cannot in any instance amount to 0″·01, and that only with regard to the smaller stars, whose proper motions have not hitherto been ascertained. The numbers in this column marked with an *asterisk,* are those which exhibit the effect of precession only.

10. *Declination.* This is taken from the same authority as the Right Ascension. Northern declinations are considered *positive*, southern ones *negative*. The sign + is uniformly omitted for the sake of distinctness.

11. *Annual Variation in Declination.* This is to be applied (for a period after 1800) according to the algebraical effect of the two signs. The remark made under the ninth column, as to the numbers marked with an *asterisk*, applies also to this column.

12. *Approximate Latitude.* It would have been highly desirable to have given the correct longitudes and latitudes of the zodiacal Stars, although these elements are of less importance than formerly, since the astronomical formula into which the positions of the stars enter, are now more frequently adapted to right ascension and declination. No catalogue however since that of Mayer, has been reduced to the ecliptic, if we except one of 600 principal stars, computed by M. Chabrol, inserted in the *Connaissance des Tems*, an xii., and thence copied into Rees's Cyclopædia, art. "LONGITUDE." Those therefore who desire to have the exact longitudes and latitudes must compute the same trigonometrically. The compiler has given the latitudes in degrees and tenths, from Flamsteed and Mayer, or else as estimated by means of a 21-inch globe; under the impression, that even in this rough manner they would assist the observer in selecting those stars, which at any particular period may be liable to occultation or appulse by the moon or a planet.

Lastly, The notes accompanying the present catalogue, are in part deduced from a comparison of the several authorities, and in part selected from the notes attached to the catalogues themselves. Assistance has likewise been derived from Sir W. Herschel's valuable papers in the Philosophical Transactions, and particularly from his catalogues of Comparative Brightness, contained in the volumes for 1796, 1797, and 1799. His descriptions of double stars are for the most part copied *verbatim* from the volumes for 1782 and 1785.

*** The compiler begs to state, that he shall be happy, in the future portions of his undertaking, to adopt any improvements that may be suggested: provided they do not interfere too much with the general plan.

Synonyms.			Character.	Constellation.	Mag.	0 hour. Right Ascen.					Declination.				Lat.
P	B	F.C.M				m	°	′	″	A.V +	°	′	″	A.V +	°
1	21			Ceti	6 7	0	0	1	6 0	46 01*	—6	21	36 6	20 04*	—5 8
1	25			——	8	1		13	51 0	46 00*	—4	26	2·0	04*	—4 3
	27			——†	7	1		19	58	46 0*	—4	10	27	04*	—3 8
16	91	35	(B)	Pisc †	6	5	1	10	18 1	46 11	7	42	36 7	05	6 6
21	92	36		——	6 7	6		34	27 1	45 91	7	7	45 1	08	5 9
26	93 M.	37		——	8	6		36	5 7	46 02*	0	44	17 2	04*	0 0
30	96	38		——†	7 8	7		46	10 5	45 93	7	45	39 3	04	6 4
33	98 M	[illegible]		——	6 7	8		52	55 5	46 02*	0	31	34 5	03*	—0 2
34	4			Ceti	7 8	8		53	38 5	45 9"	—2	58	26 2	03*	—3 1
36	14			——	7 8	8	2	1	2 1	45 97*	—3	7	33 6	03*	—3 0
12	15	8	ι	——	4	9		18	30 6	45 80	—9	55	58 5	05	—10 0
15	102	11	d	Pisc	5 6	10		34	47 3	46 24	7	4	43 6	09	5 5
60	54 M.	7		Ceti	6 7	14	3	34	9 9	45 94*	—3	19	36 5	01*	—1 5
64	107	41	e	Pisc	6	15		47	16 5	45 97	0	49	54 0	03	—0 6
65	109	13		——	6	15		50	51 3	46 10	6	35	5 8	19 98	1 5
70	56	19		Ceti	6	16	4	5	31 8	46 07	—1	9	28 3	20 03	—2 7
72	58			——†	7 8	17		13	17·7	45 86*	—6	6	37 8	19 99*	—7 3
73	110 M	16		Pisc	8	17		16	12 9	46 05*	1	42	24 5	99*	—0 1
87	65	11		Ceti	7 8	20		54	57 0	46 11	—2	13	10 5	20 07	—4 0
89	67	12	(η)	——	6	20		57	29 2	45 86	—5	3	50 2	19 99	—6 6
101	112	51		Pisc †	6 7	22	5	31	18 0	46 20	5	50	59 1	20 03	3 2
107	71 M	13		Ceti	8	23		48	48 4	45 95*	—1	12	44 7	19 95*	—3 9
110				Pisc	7	24		57	23·4	46 35*	9	12	10 6	94*	6 1
113				Ceti †	7	24	6	4	21·0	45 80	—5	39	4 1	93*	—7 2
115	123			Pisc	6 7	25		8	27·6	46 48*	12	16	11 0	93*	9 3
117	76	15		Ceti	6	25		14	16·5	46 10	—4	41	43 0	93	—6 8
120	79 M	14		——†	6 7	25		19	18 9	46 12	—1	36	20 3	92*	—1 0
131	127			Pisc	7	27		48	12 4	46 09*	2	2	12 4	90*	—0 8
133	84	15		Ceti	7	28		57	46 5	45 76	—1	36	17 0	91	—4 2
140	131			Pisc	7 8	29	7	19	4 5	46 48*	10	25	56 3	88*	6 7
146	92 M.	16		Ceti	6 7	31		37	49 0	45 76*	—5	27	3 0	87*	—8 0
				Pisc. †	7	31		43	6		8	15	24		4 5
149				——	7 8	31		47	6 0	46 58*	11	51	52 9	86*	7 9
157	99 M	17		Ceti	8	33	8	12	24 9	45 76	—4	57	16 9	84*	—7 8
167	104 M	18		——	8	35		43	18 0	45 97*	—0	50	24 3	81*	—4 2
171	105			——	6	35		48	21 3	45 70*	—5	43	37 9	81*	—8 7
178	139	57		Pisc.	6 7	36	9	1	31 5	46 83	14	22	58 7	83	9 0
179	140	58		——	6	37		9	0 0	46 51	10	52	51 1	87	6 4
183	143	60		——	6	37		15	48 7	46 21	5	38	51·7	81	1 5
186	147 M	[illegible]		——	6	38		28	31 5	46 26*	4	15	0 3	77*	0 2
190	148	62		——	6	38		28	51 0	46 37	6	12	20 3	·77	1 9
192	149	63	δ	——	5	38		34	46 5	46 40	6	29	40 5	80	2 [illegible]
201				——	8	41	10	0	23 7	46 [illegible]	6	57	24 0	73*	2 5
207	155 M	[illegible]		——	7 8	41		15	12 [illegible]	46 15*	2	17	52 5	72*	—2 0
213	134	20	(m)	Ceti	5	43		41	56 2	45 93	—2	13	59 0	71	—6 3
214	158 M	[illegible]		Pisc.	8	43		45	28 2	46 21	2	59	59 0	69*	—1 5
[illegible]	162 M.	26		——	8	[illegible]	11	11	28 5	46 11*	5	40	1 6	66*	0 9
[illegible]	164 M	27		——	7	46		25	5 4	46 92*	12	51	58 1	65*	7 [illegible]
[illegible]	[illegible] M	28		——	6 7	47		51	2[illegible] 5	46 97*	12	36	43 6	62*	6 9
[illegible]	[illegible]	[illegible]		—	7 8	[illegible]		59	21 0	46 43*	5	45	40 0	61*	0 0

Synonyms.			Character	Constel-lation.	Mag.	0 hour. Right Ascen.			Declination.		Lat.
P.	B.	F.C.M.				m	° ′ ″	A V.+	° ′ ″	A V +	°
251				Pisc.†	8	49	12 17 15·0	46 ·3*	—0 17 49 0	19·58*	—5·0
252	171	M. 30		——	6 7	19	22 1 5	46 42*	5 24 6 0	·58*	0 1
257				——	7 8	51	43 30·0	46 86	10 49 54 4	·55*	5·0
260	177	70		——	8	52	55 55 5	46 55	6 51 32 5	·54*	1 2
262				——	7	52	13 1 25 5	46 62*	7 41 37 0	53*	2·0
264	181	71	ι	——	4	53	8 37 8	46 62	6 48 37 5	52*	1 1
269	182	M. 32		——	8	53	21 17 7	46 47*	5 41 18 4	·50*	—0 0
270	155	26		Ceti †	6 7	54	22 56·1	46 11	0 17 30 2	45	—5·0
273	185	73		Pisc.	6 7	55	37 52 5	46 46	4 34 52 6	·54	—1 2
274	184	72	(ι)	——†	6	55	38 12 0	47 08	13 52 3 6	·55	7 4
280	190	77		——†	7 8	55	52 15 7	46 31	3 50 26 4	·42	—1 9
287	187	75	(H)	——†	6 7	56	14 0 49 5	47 05	11 52 50 5	51	5 5
295	161	29		Ceti †	7 8	58	25 22 5	46 23	0 56 41 0	18·94	—4 8
297	198	M. 35		——	7 8	58	28 48 4	46 79	8 50 12 9	19·41*	2 4
299	199	80	e	Pisc.†	5	58	31 13 5	46 42*	4 35 17 6	40*	—1·5
311	204			——	6	[illegible]	53 58 5	47 35*	14 36 22·7	37*	7 7
3	166	33		Ceti	6	0	15 4 6 8	46 08	1 22 40 2	37	—4 7
4	206	M. 37		Pisc.	8	0	6 37 2	46 70	8 28 58 0	35*	1 9
8	210	M. 38		——	7	1	15 19 0	46 87*	9 13 31 0	34*	2 5
10	169	34		Ceti	6 7	2	23 21·3	[illegible]	—3 18 9 0	32	—9 1
13	170	35		——	6 7	2	33 57·0	45 91	1 24 15 8	31	—4 8
16	216	86	ζ	Pisc.†	6	3	49 27·3	46 81	6 30 51 6	24	—0 2
19	217	87		——	6 7	3	52 46 5	47 51	15 1 15 1	28	7 7
23		88		——†	6 7	4	16 4 41·7	46 17	[illegible] 3 7	29	—0 9
25	177	38		Ceti	6	5	9 15·9	45 7[illegible]	—2 [illegible] 50 5	52	—8 2
28		M. 41		Pisc.†	8	5	17 21 6	46 [illegible]	5 [illegible] 12 7	21	—0 9
32	180	39		Ceti	6	6	36 52 2	[illegible]	—5 [illegible] 22 3	21	—9 8
33	181	40		——	6	7	41 18 9	[illegible]	—3 16 50 1	12	—9 6
36	226	89	ρ	Pisc.	6	7	[illegible]	[illegible]	[illegible] 30	24	—4 3
44	230	M. 43		——	8 0	9	17 13 50 [illegible]	46 [illegible]	2 31 0 [illegible]	14*	—4 7
47	181	42	(1 1)	Ceti	6	10	[illegible]	[illegible]	—1 [illegible] 15 3	21	—8 3
57	240	L. 45		Pisc.	7	11	[illegible]	[illegible]	0 [illegible] 41 6	05*	—6·5
58	193	48	(1 2)	Ceti	6 7	12	[illegible] 21 6	[illegible]	—1 29 [illegible]	08	—8 5
59	242	M. 46		Pisc.	7 8	12	[illegible]	[illegible]	[illegible]	05*	—3 7
60	243	M. 47		——	8	12	[illegible]	[illegible]	6 [illegible] 21 1	04*	—1 2
63	245	92		——	7 8	13	16 43 5	47 7[illegible]	[illegible]	01	8 3
72	255	93	(1)	——	[illegible]	[illegible]	[illegible]	[illegible]	18 [illegible]	08	9 1
77	257	94	(2)	——	6 7	[illegible]	[illegible]	[illegible]	[illegible]	18 97	9 4
83	261	95		——	7	[illegible]	[illegible]	[illegible]	[illegible] 19 [illegible]	81	—3 6
84	262	M. 50		——	7	[illegible]	25 0 0	[illegible]	[illegible]	90	7 3
85	263	C. 22		——†	7	[illegible]	[illegible]	[illegible]	6 [illegible] 11 0	90	—1 2
91	264	96		——	6 7	[illegible]	[illegible]	[illegible]	[illegible]	86	—1 9
92	265	97		——	6 7	[illegible]	[illegible] 18	[illegible]	17 19 [illegible]	·86*	8 3
95	266	98	μ	——†	5	[illegible]	[illegible]	[illegible]	[illegible]	60	—3 1
98	267	99	ν	——	4	21	12 2 4	[illegible]	[illegible]	86	5 1
101	268	M. 53		——	7 8	[illegible]	[illegible] 21 0	47 22	[illegible] 12 [illegible]	80*	1 2
				——†	10	[illegible]	[illegible]		[illegible] 56 3		6 9
107	269	M. 54		——	7	[illegible]	42 [illegible]	[illegible]	[illegible] 10 [illegible]	75*	—1 1
110	271			——	6	[illegible]	0 12 0	[illegible]	[illegible]	71*	8 1
111	272	100		——†	7	[illegible]	[illegible]	[illegible]	[illegible]	76	2 5

Synonyms.			Character.	Constel. let. n.	Mag.	I hour Right Ascen					Declination				Lat.
P	B.	F.C M				m	°	′	″	A.V.+	°	′	″	A.V.+	°
114	271	m. 55		Psc.	8	21	21	6	3·9	46·93*	7	14	16·2	18·70*	—1·5
	230			Cet	7	25		7	4·0	46·0*	—0	5	5·9	7*	—8·4
118	275	101		Psc	6	25		15	25·2	47·67	13	38	3·9	·73	4·3
120	276			—	6	25		16	45·3	48·15*	16	24	18·6	68*	7·1
123				—	6 7	26		23	56·2	46·86*	6	37	2·3	66*	—2·1
126	279	102	τ	—	6	27		37	11·1	47·37	11	6	50·8	·71	1·9
128	281	m. 57		—	7 8	27		45	45·0	47·46*	11	3	15·2	61*	1·8
133	284	103		—	7 8	28	22	7	27·0	48·03	15	36	22·3	·62	5·9
136	286	104		—	6 7	29		8	28·5	47·98	13	15	57·6	57	3·7
138	287	105		—	6	29		13	42·3	48·18	15	23	10·6	59	5·6
141	288	m. 58		—	7	30		30	52·5	47·05*	7	44	30·2	51*	—1·6
149	290	m. 59		—	8	31		43	30·0	47·11*	8	3	20·0	·48*	—1·4
150	291	106	ι	—	5	31		45	31·8	46·61	4	28	14·5	47	—4·7
154	292	107		—	5 6	32		51	57·0	48·41	19	17	30·0	17·87	9·0
				—	9	34	23	22	7		8	29	1		—1·2
162	294	108		—	6 7	34		30	27·3	48·66	19	4	44·5	18·33	8·6
164	298	110	ο	—	5	35		42	41·7	47·29	8	8	46·7	39	—1·6
	3	3		Aq.	Var.	36		56	21·5	48·41*	16	21	18·8	32*	6·0
169	300	m. 60		Psc	8	37	24	8	21·0	47·43*	9	50	23·0	29*	—0·2
172	5	4		Aq.	6 7	37		20	21·0	48·44	15	57	15·0	30	5·1
174				—	8	38		23	6·0	48·39*	16	1	2·5	25*	5·5
175	302	c. 53		Psc	7 8	38		31	15·0	46·40*	2	10	55·2	24*	—7·0
179	8			Aq	6	39		46	43·5	49·28*	21	16	35·6	·20*	10·0
185	15	51	(ι)	Cet Aq	6	40	25	3	57·9	47·20	10	2	50·8	10	—0·4
191				Cet	7 8	41		21	30·0	47·50*	9	49	1·0	11*	—0·6
197	16	5	ν	Aq	4 5	43		38	43·8	48·99	18	18	28·5	04	7·2
201	308	111	ξ	Psc	5 6	43		48	12·6	46·37	2	11	12·0	17·95	—7·9
202	18	6	β	Aq	3	43		51	12·6	49·14	19	49	29·0	97	8·5
209	309			Psc	7	46	26	33	36·5	46·14	0	51	11·9	95	—9·5
214	21	8	ι	Aq.	6	46		55	11·1	48·82	16	50	6·5	90	5·4
222	27			—	6	49	27	7	16·5	49·31*	20	4	52·7	84*	8·4
223	26	m. 68		—	7	49		11	5·1	47·81	11	19	9·5	83*	0·1
225	311			Psc	7	49		22	37·5	46·83*	5	3	31·5	80*	—5·8
226	312	112		—	6	50		26	21·0	46·69	2	8	4·0	53	—8·6
227				Cet	7 8	50		25	55·0	46·97*	5	56	34·4	79*	—5·0
228				Psc.	7 8	50		29	10·0	46·55*	3	24	46·8	78*	—7·4
234				Cet	7 8	51		49	48·0	47·14*	0	53	44·4	72*	—4·2
238	313	113	α	Psc	4	52		55	39·1	46·45	1	47	32·8	75	—9·1
240	34			Aq	7	52	28	4	37·8	47·08*	10	2	56·0	69*	—1·3
245	35		λ	—	6	53		11	27·0	48·96	17	17	9·2	67*	5·4
249	39			—	7 8	54		34	12·3	47·15*	6	46	13·8	60*	—4·7
[illegible]	42	12	κ	—	6	55		51	0·0	49·84	21	41	20·3	59	9·2
[illegible]	45	1	[illegible]	—	3	56		56	54·0	50·22	22	30	36·5	·44	10·0
257	47	7		—	8	57	29	12	7·2	49·02*	17	4	13·5	·49*	4·8
				—	8	59		39	7		19	23	7		6·8
[illegible]	320			Cet	6 7	59		48	54·7	46·58*	3	16	46·3	·39*	—8·4
262	50	1		Aq	6	0		53	24·9	49·38	18	53	1·0	·38	6·0
1	53	r. 75		—	8	0	30	2	47·1	49·75*	20	25	42·7	·35*	7·7
6	330	64		Cet	6 7	1		12	8·4	47·23	7	37	37·0	·22	—4·4
11	[illegible]	17	[illegible]	Aq	6	2		21	27·5	49·84	20	15	52·6	36	7·4

Synonyms			Character.	Constellation.	Mag.	11 hours Right Asc.					Declination +				Lat.
P.	B.	F.C.M.				m	°	′	″	A.V.+	°	′	″	A.V.+	°
12		18		Ari. †	8	2	30	27	21·3	49 19*	18	52	34·4	17 28*	6 1
15	58	19		——†	7	2		32	27·9	48 61	14	20	8·8	·27	1 8
16	336	65	ξ. 1	Ceti	5	2		36	12 4	47 33	7	54	8 0	·25	—4·3
20	59			Ari.	8	3		41	53·7	49 47*	18	40	15·1	23*	5 9
23	337			Ceti*	7·8	3		45	46 5	46·69*	4	4	20 3	22*	—8·0
49	67	22	ϑ. 1.	Ari	6	7	31	45	22·0	49 61	18	58	8 4	08	5 7
54	71	23	ϑ. 2.	——	7·8	8	32	0	44·4	49 55	18	45	53 2	16 90	5 5
63				Ceti	7 8	11		44	30 0	47 31*	6	49	46 8	86*	—6·0
75				——	6 7	14	33	27	37 2	47 72*	8	48	9 0	·74*	—4 4
76	75	24	ξ. (1)	Ari.	6	14		31	43 0	47 90	9	41	52 5	·69	—3 6
82				——	8	15		44	51·0	47 89*	9	35	42 8	66*	—3 5
83				——	7 8	15		51	55·5	47 93*	9	41	30 3	61*	—3 6
85				——†	6	16	34	0	45 9	47 92*	9	39	28 6	·61*	—3 1
91	82	25	(ξ 2)	——†	7 8	17		11	36·3	47 68	9	18	13 0	·40	—4 2
94	382	73	ξ. 2	Ceti	5	18		23	8 5	47 57	7	33	22 0	·52	—5 9
	389			——†	7	19		43	24	47 8 *	8	40	1	·5*	—4 9
96	87		(f)	Ari. †	6 7	19		45	55 5	51 19*	24	20	26·5	·46*	9 9
98	88	26		——	6 7	19		51	43 8	49 93	18	57	36 2	50	4 7
101	89	27	(ψ)	——	6	20		57	31 5	49 50	16	48	44 0	·41	2 7
109	91	29	(ω)	——	6 7	22	35	29	33·0	48 89	14	8	29 8	·38	—0 0
112				——†	6 7	22		36	46 5	49 80*	17	59	25 1	·29*	3 7
118	405			Ceti	6·7	24	36	7	25·5	47·38*	6	35	37 5	·19*	—7 4
123	407			——	6·7	25		16	56·2	47·24*	5	55	29 7	16*	—8 0
125	410	78	ν	——	4 5	25		20	53·5	46·82	4	42	48 0	16	—9 2
128	95	30		Ari †	6·7	25		22	33·0	51·42	23	46	5 1	13	8 8
129	96	31	(ν)	——†	6	26		26	11 1	48·73	11	34	22 9	11	—2 7
130	412			Ceti	6 7	26		30	3·7	47·44*	6	31	7 0	·11*	—7 2
135	418			——	7 8	27		51	0 0	47·45*	6	19	19 4	·05*	—7 3
136	98	32	ν	Ari	5 6	27		52	16 5	50 62	21	5	17 0	·02	6 1
140	421			Ceti †	8	28	37	4	30 0	48 09*	9	46	0 0	·00*	—4 6
148	427			——	7	30		25	54 0	47 13*	5	14	35 7	15 92*	—9 0
151				——	7 8	31		39	12 9	47 13*	5	12	22 0	87*	—9 2
153	104	34	μ	Ari	6	31		46	37 5	50 16	19	9	5 0	86	1 0
155	434			Ceti	7	31		48	53 7	48 11*	9	41	0 0	84	—1 9
156	438	85		——	6	32		55	59 1	48 08	9	52	55 3	88	—4 8
162	108	36		Ari	7	33	38	17	39 0	49 82	16	51	32 9	·73	1 7
164	109	37	ο	——	6 7	34		23	12 0	49 17	14	27	26 0	·75	—0 6
166	110	38		——†	5 6	34		31	9 0	48 63	11	35	47 0	64	—3 4
167	449	87	μ	Ceti	4	34		32	10 5	48 25	9	15	44 0	71	—5 6
181	116	16		Trian } Ari. }	6 7 *	37	39	17	25 5	51 75	24	20	40 8	54	8 5
182	117	40		——	6	37		20	9 0	49 98	17	26	34 1	61	1 9
185	118	42	π	——†	5	38		32	13 5	49 70	16	37	26 5	·52	1 1
192	122	43	σ	——	6	40	40	7	0 0	49 18	14	11	59 0	·33	—1·3
203	124			——	7	42		31	11 2	49 66*	15	39	31 5	·24*	0 0
210	129	44	ρ. 1	——	7 8	44		55	56 1	49·95	16	54	48 0	·22	1·0
212	130	45	ρ. 2	——	6	45	41	8	50 7	50·09	17	30	47 0	·17	1·5
213	131	46	ρ. 3	——†	6	45		17	33 0	50 45	17	12	59 0	14 89	1·2
215	475			Ceti	6 7	46		23	15·0	47·90	7	34	6 7	15 01	—8 2
218	132	47		Ari. †	6	47		39	49·5	50·84	19	51	28 9	·07	3 6
				——†	7	47		51	38·	51·09*	20	48	30	14 93*	4 5

Synonyms.			Character.	Constel-lation.	Mag.	II hours. Right Asc.					Declination. +				Lat.
P.	B.	F.C.M.				m	°	′	″	A.V.+	°	′	″	A.V.+	°
224	136	48	ι	Ari.	5	48	41	57	0·0	51 04	20	31	54 2	14 96	4 2
228	481	91	λ	Ceti	5·6	49	42	15	3 6	47 93	8	6	6 0	·84	—7 8
230	139	50		Ari. †	7·8	49		19	33·7	50·11	17	12	6 0	·82'	0 7
233	141	49		——	6	50		32	15·0	52 47	25	39	41·0	·76	8 9
235	142	51		——	7	51		39	3 0	52·58*	25	49	9 0	·74*	9 0
250	146	52	(h)	——	6 7	54	43	26	4 0	52 13	24	28	1·6	·56	7·5
257	148	53		——	6	56	44	2	50 7	50 22	17	5	54 5	·41	0·3
259	149	54		——	6 7	57		15	31 5	50 46	18	1	5·0	·38	1 1
261	151	M. 98		——	7	58		28	21·6	51 10*	19	59	11 4	31'	2 9
262	498			Ceti	7	58		29	14 1	47 91*	7	41	30 2	·30'	—9 0
264	153	M 99		Ari.	8	58		33	24·3	50 75*	18	36	24 8	·29*	1 6
				—— †	6 7	59		38·1		52 92*	26	7 2		26*	8 7
	157			—— †	7 8	0		57	7	51 5*	21	24	11	·2*	4 2
2	158	57	δ	——	4 5	0	45	3	14·4	51 09	18	57	37·5	·20	1 8
3	159	56	(i)	——	6	0		5	12 0	52·97	26	29	34 6	·16	9 0
4	3			Tau †	6 7	0		6	0 9	49 10*	12	16	49 0	·15*	—4·6
11	163	58	ζ	Ari.	5	3		51	24 0	51·24	20	17	37 0	13 93	2 9
29	164	59		——	6 7	8	47	0	10·5	53 16	26	20	16 0	64	8 4
34	167	60		——	7 8	9		8	56 2	52 78	24	55	55 3	·57	7 0
				—— †	7 8	9		13	51		18	59	45		1 3
38	169	c. 75		——	8	9		20	42·0	51 32*	19	46	37·2	·59'	2 0
40	170	61	τ. 1	——	6	10		25	35·4	51·56	20	24	58 5	55	2 6
42	171	62		——	6	10		33	16·0	53 45	26	52	44·4	·54	8 8
45	172	63	τ. 2	—— †	7	11		48	57·0	51 25	20	0	57·7	·44	2 1
49	173	64	(g)	——	5·6	13	48	7	54·0	52 66	24	0	22 0	·42	5 9
				—— †	8	13		10·7			27	56 2			9 7
50	174	65		——	6	13		13	48 6	51 34	20	5	2 3	42	2 1
55	9	1	ο	Tau. †	4	14		30	58 2	48 15	8	18	56 1	·24	—9 4
60	175			Ari.	7	16		54	51 9	50 94'	18	2	45 3	·18'	—0 1
63	12	2	ξ	Tau.	4	16	49	5	10 8	48 46	9	1	33 5	·12	—8 8
				Ari. †	8	16		6			19	45 5			1 6
65	177	66		——	6 7	17		11	39·6	52 21	22	6	17 0	·00	3 8
67	13	M. 108		Tau.	7 8	18		25	23 4	48 89*	10	41	20 5	·04*	—7 3
70	15	M. 109		——	8	18		36	33 6	50 41*	16	3	46 6	12 99'	—2 2
75	18	4	s	—— †	6	19		32	22 5	48 91	10	38	28 0	·97	—7 5
77	20	5	f	——	5	20		57	42 3	49 36	12	14	29 5	·90	—5 9
82	23			——	8	22	50	25	24 0	50 42*	15	54	56 5	·77*	—2 5
83	24	6	t	——	6 7	22		26	51·9	48 37	8	41	16 0	·73	—9 5
86	25	7		—— †	6	23		39	15·0	52·72	23	46	58 0	·69	5 0
87	26	M. 113		—— †	7 8	23		41	28 5	50 81*	17	9	58 8	·70	—1 4
	29	9		—— †	Var.	25	51	18	29 6	52 51*	22	32	21·7	·53*	3 7
				——	9	26		29·0			17	55·5			—0·8
99	30	M. 114		——	7 8	27		38	50·1	50·16*	14	45	47·3	·44'	—3 9
103	33	c. 87		—— †	7	28	52	2	0 6	50·51*	15	52	32·5	·33'	—3·0
	34			—— †	6	29		8	49	54 8*	28	7	29	·3*	9 0
107	36	11		——	6	29		12	45·6	53·27	24	40	16 5	·27	5·5
115				——	7 8	31		37	49·8	52 50*	22	8	17 0	·17*	3 0
118	40	13	(F,1)	——	6·7	31		42	1·5	51·48	19	2	56 7	·17	—0 0
				—— †	7·8	31		47·4			25	2·2			5 9
125	47	14	(F.2)	——	7	32	53	3	36 0	51 62	19	1	19·	0·7	—0·1

Synonyms.							III hours.	Right Asc				Declination +				Lat.
P.	B.	F.C.M.		Con-stellatio			m.	°	'	"	A.V. +	°	'	"	A.V.+	°
128				Tau.		7 8	33	53	12	52 5	51 95'	20	17	8 5	12·01*	1·1
129	52	16				5 6	33		11	3·0	53 06	23	38	57 0	11 96	4·3
130	53	17				4 5	33		15	21 3	53 01	23	28	26·0	12 03	4·2
131	56	18				7			18	46 8	53 24*	24	12	0 5	11·99*	4 8
132	57	19				5			19	53 4	53 07	23	49	43 9	·99	4·5
133						8 9	34		24	0 0	52 70*	22	34	2 1	·95*	
135	60					7 8	34		26	19 [illegible]	08*	23	41	54 0	·94'	
136	61	20							29	13 8	53 07	23	43	53 8	·93	4 4
137	63	21				7·8			30	12 0	14	23	55	4 5	86	4 5
139	64								30	21 3	52·09*	22	30	41 5	·92*	
141	65	22		P		7 8	34		32	21 0	53 09	23	53	32 0	·91'	4 5
144	68	23			†	5	34		37	11 7	53 07	23	18	56 0	·09	3 9
147	74				†	7 8	35		40	20 2	3·18*	23	53	17 7	·85'	
150	86	24			†				52	13 5	2 95	23	29	9 6	82*	4 0
151	94								54	11 8	3 11'	23	39	32 0	·81*	
152	93				†	6			54	16 3	3 02	23	28	31 0	·77	4 0
	104				†		37	54	7	58 7	2 81'	22	47	44 0	·74'	
153	105						37		9	13 9	3 16*	23	43	13 0	·74*	
156	108	26					37		16	18·9	2 91	23	14	2 0	·71*	3 7
157	112	27					37		19	22·5	3 02	23	25	49 6	·64	3 9
158	113	28					37		19	37·0	3 01	23	30	50·3	·63	4 0
159	114	30					37		19	41·1	48·89	10	31	3 4	·68	—8·7
161							37		20	12·0	53·02*	23	15	51 0	69*	
163	126						38		28	9·0	2·97*	23	5	26·0	·65*	
164	131						38		30	46·5	53·19*	23	43	47·7	·64*	
165	132						38		31	38·2	3·02*	23	13	41 0	63*	
166	133	c 100					38		32	45 0	2·49'	21	37	28 0	·63*	2 2
170		c 101					38		34	42 9	53 62*	24	57	51 0	·62*	
171	138						39		38	27·9	25*	23	52	37 5	·60*	
172	140						39		45	3 6	53 08*	23	20	41·5	·57'	
175	141						39			53 2	3 50*	21	32	47 1	·54*	
179	144						40	55		35 4	3·53*	24	33	28 7	·48*	
187	148	a. 129					42			9 0	0 97*	16	43	13 0	·37*	—2 9
197	153	32								3 6	2 71*	21	53	24 0	·13*	2·0
199	155	33								19 5	2 95*	22	54	58 6	·12*	2 6
213	161									15·0	2 85	22	37	33 5	10 85	2 6
214	162		(G)							5 1	1 08*	6	43	12 7	·83*	—3 2
215	163								19	39 0	1 37'	7	37	7 0	·82*	—2 3
217	164								22	6 0	1 77	9	37	36 0	·81*	—0 5
218	166	35							24	10·3	9 50	1	54	54 3	·88	—8 0
	168	36						58	6	18 6	3 44	23	32	36 5	·59	3·2
	170	37	a.(1)						13	21 6	2 78	21	31	23 1	·47	1 2
236	174	39	(a.2)						22	45 0	2 94	21	27	27·0	13	1·1
243	177	41							35	28 5	4 83	27	2	54 5	42	6 6
245	178	42	ψ						40	0 0	5 23	28	26	55 2	33	7 9
249	180							59	8	17 2	1 21*	6	47	37 0	28*	—3 5
252	182	43	ω. 1						23	0·7	2 12	9	1	11 4	·29	—1 4
254	184								28	3·0	9 95*	2	51	26·6	18*	—7 4
256	186	44	p						40	5·4	4 39	25	50	45 0	·05	5 3
261	190								54	55 5	1 03*	6	6	47·5	05*	—4 2

NOTES.

NOTES.

B. 27 Ceti.) Is not in Piazzi's own Catalogue, although inserted in Bode's as from an observation of the former, and marked double. It must be Herschel's star II. 55, which in Bode's note is erroneously referred to 14 Ceti. "About 1° s. following 4 and 5 Ceti in a line parallel to η and τ; in the shorter leg of a rectangular triangle*. Very unequal. L. r; s. d. With 278, rather more than 2 diameters. Position 21°·7 n. preceding."

35 Piscium.) Double. Hers. III. 62. The following star, 17 of Piazzi. R.A. + 9″·7. Decl. −12″·0. "Considerably unequal. L. r.w. S. p.r. Distance 12″·5. Position 58°·9 s. following."

38 Piscium.) Double. Hers. II. 50. "Pretty unequal. Both pr. With 227, full 2 diameters of L, with 460 about 4 diameters. Position 25°·5 s. preceding."

*P.*O. 72, or B. 58 Ceti.) Bode's declination is +3′.

51 Piscium.) Double. Hers. IV. 70. "Very unequal, L. r.w; S. d. Distance with 278, 22″·5. Position 0°·6 n. following."

M. 14.) This star was observed by Flamsteed, and is 312 of Miss C. Herschel's Catalogue. An error of 3° in the declination no doubt occasioned the insertion of 14 Ceti in the Brit. Cat., and this probably typographical. (See the Additions and Corrections at the end of Wollaston's *Fasciculus*.)

Anonymous. R.A. 7° 43′.) Position from Lalande. *Histoire Céleste,* page 127. It is C. H. 185.

*P.*O. 251.) Double according to Piazzi. The other star of 9th mag. R.A. − 1′. Decl. + a small quantity.

26 Ceti.) Double. Hers. IV. 83. "Very unequal. L. r.w. S. d.b. Distance 17″·03 mean measure. Position 14°·6 s. prec."

72 Piscium.) Bode's declination is +5′.

77 Piscium.) Double. Hers. IV. 68. "A little unequal, L. w.r.; S. p.r. Distance 29″·6. Pos. 4°·8 n. foll. not accurate." The following star 281 of Piazzi. R.A. +38″3; Decl. +2·1. mag. 8.

75 Piscium.) The Brit. Cat. requires + 16′ in [illegible]A.

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* The compiler of the present catalogue has thought it proper to give Sir W. Herschel's descriptions more fully than has been done either by Wollaston or Bode; and there is the more reason for this, since the *second* catalogue in the Phil. Trans. for 1785 is so extremely curtailed in the abridgement of that work, as to be nearly useless. In the descriptions, L signifies the larger star, S. the smaller, w. white, r. red, d. dusky, p. pale, b. blue.

29 Ceti.) The annual proper motion by good obs. of Bradley and Piazzi is +0″·14 in R.A. −0″·47 in decl.

80 e Piscium.) seems to have a considerable proper motion.

86 ζ Piscium.) Double. Hers. IV. 8. The foll. star, 17 of Piazzi. mag. 8. Diff. of R.A. Hers. +20″·6. Pi. +17″·7.—Decl. +8″·5, and 8″·4 respectively. "Pretty unequal. L. w; S. w. inclining to blue. Distance 22″·2, not very accurate. Pos. 22°·6 n. following."

88 Piscium.) The position of Bode's 220, which he makes synonymous with this, requires a correction of −8′ in R.A. and −7′ in decl.

P. I. 28.) Piazzi anonymous. Mayer's star 41 is not in Wollaston's Cat., but it is found among the additions at the end of his *Fasciculus.* The R.A. there given requires a correction of +5′ and the declination +30½′ to make them agree with Piazzi.

C. 22.) Piazzi anonymous, and double. The following star (87) mag. 9·10. R.A. +67″·2. Decl. −8″·7.

98 μ Piscium.) Near this to the West, a double star, *Bode.* Can this be the star referred to in the preceding note?

Anonymous. R.A. 20° 30′.) From *Hist. Céleste,* p. 192, supposed to be Herschel's double star IV. 130. "About 1½° n. of, and a little following η Piscium, in a line parallel to β Arietis, and β Trianguli; the last of four in a crooked row. Very unequal. L. r; S. darker r. Distance with 278, 15″·8. Pos. 62°·25 n. following."

100 Piscium.) Double. Hers. IV. 131. Following star, Pi. 112, mag. 8. R.A. +18″·0. Decl. +2″·2. "Pretty unequal. L. p.r. S. r. Distance 15″.87. Pos. 5°·0 n. following."

B. 230 Ceti.) Position from Lalande. It is C. H. 329.

P. I. 123.) Another star of 6th mag. follows, about 8′ north.

102 π Piscium.) Flamsteed's number is erroneously quoted 120, by Piazzi, who concludes the star to be variable, since Flamsteed and Lacaille set it down of the 5th mag., and Mayer of the 4·5.

106 ν Piscium.) Is the same with 51 Ceti of the Brit. Cat.

107 Piscium.) It is generally considered that this star is identical with 2 Arietis; yet Herschel observed two stars in the place, [illegible] brightest of which he took to be 2 Arietis.

Anonymous. R.A. 23° 23′.) This may probably be Herschel's double star II. 49. "About ½° n. of, and a little preceding 110 Piscium, towards η. A little unequal. Both w.r. With 460, about 3 diameters of L. Pos. 59°·1 n. preceding. A third star in view, about 1¼′." *Histoire Céleste,* p. 41.

M. 57.) Is 28 of Lacaille Zod. Cat., and so denominated in Piazzi.

M. 62.) Piazzi, anonymous.

3 Arietis.) Is undoubtedly variable, since Piazzi could not see it, although observed by Flamsteed, Bradley, Herschel, and Bessel. According to Herschel's estimate, it must have been a bright 7th mag., and Bessel sets it down as of 7·8. The position here given is from Bradley.

P. i. 174.) Piazzi supposes this to be 3 Arietis, as he could not find any other in the spot. But see the preceding note. Is not this Herschel's double star V. 92, which he describes as "full $\frac{1}{2}^\circ$ s. foll. 3 Arietis, in a line parallel to α Arietis, and δ Ceti; the most south of two. Equal. Both reddish. Dist. 51″·27. Position 52°·75 n. preceding, or s. following.

P. i. 179, or *B*. 8 Arietis.) Is C. H. 143. Double. Hers. I. 73. "About $1\frac{3}{4}^\circ$ n. prec. β Arietis, towards β Andromedæ; a considerable star. Very unequal. L. r; S. deeper r. With 227 about $\frac{1}{4}$ diam. of L; with 460, almost $1\frac{1}{2}$ diam. Pos. 77°·4 s. following."

54 Ceti or *B*. χ Arietis.) Is 63 of Mayer. The R.A. of the Brit. Cat. requires —15′.

5 γ Arietis.) The first star of Aries from which the longitudes of other stars were reckoned by the old astronomers. The name given it is Mesartim. Double. Hers. III. 9. The other star, 196 of Piazzi, same magnitude and R.A. Decl. +8″·9. A third star 10 mag. following. "Equal, or if any difference the following is the larger. Distance 10″·172, a mean of 2 years' obs. L. w. inclining a little to r. S. w. Pos. 86°·1 n. prec."

6 β Arietis.) "Almost 1° n. preceding this, towards ζ Andromedæ, a small double star." Hers. II. 56. "A little unequal. Both reddish. With 227, full 2 diameters of L. Position 23°·2 n. preceding. A third star 2′ or 3′ preced. in the same direction with the 2 stars of the double star."

P. I. 209.) Piazzi in a note says, Double, Hers. V. 84; but the star of that class and number is 47 Cassiopeiæ, which precedes this, viz. No. 208.

P. I. 222, or *B*. 25 Arietis.) Is C. II. 106.

112 Piscium.) Proper motion in R.A. +0″·33, in decl. —0″·26, by Br. and Pi.

P. I. 227.) Either there is an error in the R.A. of this star, or it is out of its proper order.

113 α Piscium.) Double. Hers. II. 12. "Considerably unequal. Both w. With 222, not quite 2 diameters of L; with 460, about 3 diam. Dist. 5″·123 mean-measure. Pos. 67°·4 n. preceding."

Anonymous. R.A 29° 40.) Supposed to be Hers. double star III. 68. *Hist. Céleste*, p. 33. "Full one degree south, prec. η Arietis, in a line parallel to α and γ. Very unequal. L. p.r.; S. d. Dist. 8″·1. Pos. 55°·7 s. following."

M. 75.) Piazzi anonymous.

P. II. 12.) Piazzi anonymous. Lalande calls it 18 Arietis, and it is presumed to be the star whose brightness Sir W. Herschel has estimated by that appellation. In this case the Brit. Cat. requires —2′ in R.A. and —6′ in Declination.

19 Arietis.) The R.A. in Mayer, 78, requires —1°.

24 and 25 Arietis.) Between these, according to Bode, are three stars, one of which is double. These can be no other than 82, 83, 85, of Piazzi, the latter is C. H. 330. 25 Arietis has a proper motion of —0″·18 both in R.A. and Declination.

B. 389 Ceti.) Is C. H. 187. The position is from Lalande.

P. II. 96, or *B.* f. Arietis.) Double. Hers. M.S. Sept. 1784.

78 ν Ceti.) Proper motion —0″·17 in R.A.

30 Arietis.) Double. Hers. V. 49. Preceding star, 6 mag. 126 of Piazzi, who makes the diff. of R.A. —48″·0 and of Decl. +0″·9. Bradley, diff. R.A. —42″·8. Proper motion of the following star +0″·17 in R.A. "Nearly equal. Distance 31″·1 inaccurate."

31 Arietis.) Piazzi anonymous. Pr. motion in R.A. +0″·28.

⁂ The remainder of the Notes tot his portion, and a List of such stars of Wollaston's Catalogue as are not contained in this Catalogue, with the reasons for their omission, will be given with the next portion.

XXVIII. *On the Appearance of Meteors as Prognostics of Wind and Rain. By Dr.* W. Burney.

To Dr. Tilloch.

Gosport Observatory, Aug. 13, 1821.

Sir,—I herewith send you the inclosed meteoric observations made here, which will be found to illustrate many points advanced merely upon supposition in my article on Meteors in your last Number; particularly in respect to the little effect the light afforded by the Moon in her second quarter has at this time of the year; or that of Jupiter, in obscuring the small and second-sized meteo[illegible] I also send you the barometrical observations made this day at the proposed hours, and am, sir,

Yours truly,

William Burney.

July 12th 1821. At a quarter past 9 P.M. a light red meteor of a large size, and of a spheroidal shape, appeared in its course from the zenith towards the North: its track, which was 35° in length, formed an angle with the horizon of about 60°, and a retardation was observed in its motion just before it disappeared. In 2½ days afterwards, heavy rain and wind came on from S.W. and W.

July 21st. Between 9 and 10 P.M. two brilliant meteors appeared—one inclined to the eastward nearly in the direction of the wind; the other was opposed to it, and passed between Arcturus and α in the Northern Crown. At this time loose patches of *cirrostratus* were observed in different parts of the sky, succeeded in the night by heavy rain and wind

— 22d. Two small lofty meteors appeared to the eastward at 11 P.M. A copious dew [illegible] in the night. The following day was marked by variable winds that terminated in a very strong southerly gale, which brought up heavy rain from that quarter.

— 27th. From a quarter before till a quarter past 11 P.M. four meteors appeared—the lowest and largest of these, at five minutes before 11 o'clock, descended in a southerly direction, immediately under the constellation Bootes: both the head and train were red, the latter about 15° long, accompanied by a hissing, like that of a sky-rocket in its ascension, and did not disappear till a second of time after the extinction of the former. The sky at the same time was filling with dense *cirrostratus*, and soon became overcast. The following day opposite winds and light rain occurred.

— 28th. At half past 10 P.M. a small meteor passed under Dubhe, in Ursa Major, and left a whitish train behind it, about two seconds after the body had disappeared. From that time till 12 o'clock, eight other meteors, nearly of the same height, appeared without trains, viz. two under the Northern Crown, and one on each side of it, one over Jupiter and Saturn, one near the Pleiades, and two in the brightest part of the Milky Way to the southward. Strong gales from the S.W. happened on the two subsequent days.

Aug. 3d. From 9 till half past 10 P.M. five meteors shot in different directions, two of them had long sparkling trains, which disappeared with the meteors—the largest of these having been formed in the lower atmosphere to the southward, cast a white light on the ground. While these meteors appeared, a pretty white level *stratus* rose from the grass-fields and surrounding lakes, and was followed by a dense fog throughout the night.

— 4th. From 9 till 12 P.M. sixteen small and middle-sized meteors appeared in various parts of the sky, which was apparently cloudless; six of these had very long luminous trains, and some of them continued to issue sparks after the bodies had disappeared. They were of various colours, as white, red, and a mixture of light blue and light red. Four of these trained meteors were thus traced in their flight between 10 and 11 o'clock; one through the Northern Crown; one under Sagittarius; one between Aloth and Benetnasch in Ursa Major, and one between Saturn and Jupiter, whose approximation is now so conspicuous.

Aug. 5th. From 9 till 12 P.M. twelve meteors appeared, five of which had long trains: the largest of these, at forty minutes past 10 o'clock, was of the apparent size and colour of Jupiter, and passed through a space of about 26°; viz. from between the stars *α* and [illegible] Draco, thence under Alioth to Cor Caroli; its train was about 20° long, and threw off inflammable sparks a short time subsequent to the disappearance of the body. Fresh breezes and light showers of rain followed the next day.

—— 6th. At ten minutes before 9 P.M. a brilliant meteor descended almost perpendicularly and within 1½° of the moon's northern limb. This was the nearest meteor I have ever seen after her first quarter, and when shining in a cloudless space. Rain and a strong gale from S.W. followed throughout the day and night of the [illegible], and storms on the day of the 9th.

—— 9th. From 10 till 12 P.M. ten meteors appeared, while the moon shone bright in the middle of her second quarter, so that her light at that age was not sufficient to obscure the smallest and brightest of these, of which one exhibited a long train, and passed between Pisces and Pegasus at a quarter before 12 o'clock. The sky was apparently clear, but there was haze around the horizon, and a brisk gale from the westward at the time of their appearance.

—— 10th. In the course of the evening four small meteors appeared, three of these to the northward. On the 11th the gale prevailed, with showers at intervals.

—— 11th. About 10 P.M. two brilliant meteors appeared: the first, which inclined to the South, was even seen through an extensive cloud with a quick motion; the other towards the North, of a bluish colour, advanced comparatively slow in almost a horizontal direction, and left a short train behind it. Rain and wind came on in the afternoon of the 13th.

[To be continued.]

Dr. Forster observes in his article on Meteors, in your Number for June, page 419, that the kind of meteors distinguished by leaving long trains of light behind them, almost always forebode windy weather. This is strikingly verified by the above observations, with the addition of rain. The Doctor further observes, "When they occur, I have noticed that all meteors which happen on the same evening leave the aforesaid long white tails behind them." In these observations we do not agree on this point.

We have in former observations frequently seen a few meteors with and several others without trains of light the same evening; and yet they have generally appeared of the same size, height, and colour, and nearly in the same tract. If the properties of the atmosphere about East Grinstead are different from those about Gosport, that might account for it: but this is not probable in so short a distance. Certainly we have a greater exposure of water around us here, and a greater quantity must necessarily be carried up by evaporation than in the more inland parts of the country: but whether this would alter the chemical properties of the atmosphere, so as to give a different aspect to our view of meteors in motion, appears questionable. We have no other object in view by these remarks, than to establish the facts of observations on these and other atmospheric *phænomena*, which have been made here occasionally with the greatest caution; and therefore think, with all due respect to Dr. Forster, who appears to be an acute observer, that the above quoted assertion does not *always* hold good throughout whole evenings observations, even when wane-clouds, or small and thin cirrostrative *nubeculæ* abound; nor yet when the sky is apparently clear. If the Doctor can conveniently devote a few clear nights when meteors prevail, we have no doubt that he will see the propriety of these remarks.

XXIX. *A Refutation of Mr.* HERAPATH's *Mathematical Inquiry into the Causes, Laws, &c. of Heat, Gases, Gravitation, &c. By Mr.* THOMAS TREDGOLD.

> "We are to admit no more causes of natural things than such as are both *true*, and sufficient to explain their appearances."
>
> NEWTON. *Rules of Reasoning.*

To Dr. Tilloch.

SIR, — IN some of the late Numbers of the Annals of Philosophy* there has appeared an attempt to account for the phænomena which are commonly ascribed to an *attractive force*.

Such attempts are naturally to be expected, and will continue to be made so long as men feel desirous of comprehending the nature of the material world. It is also equally natural to oppose new doctrines till their principles shall be shown to be without objection; therefore, I beg of Mr. Herapath to consider with attention the following remarks, and to consider their sole object to be the advancement of knowledge.

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* Annals of Philosophy for April, May and June 1821

We are utter strangers, but we meet on the same area in the most honourable species of contest that human powers can be engaged in; let it, therefore, be conducted with fairness and honour.

The first point to which I would direct Mr. Herapath's attention is, that the mere solution of certain phænomena, or ascertaining the ratio of certain forces, is not a sufficient reason for adopting a new hypothesis, unless the assumed principles be consistent with universal experience.

If the assumed principles, or any one of them, be not true, every conclusion derived from those principles must fall to the ground.

If any one of the assumed principles cannot be directly established by a reference to unquestionable experience, the conclusions must remain hypothetical, notwithstanding that an explanation of some phænomena may be effected by such hypotheses.

On these grounds, which I think are unobjectionable, I must reject Mr. Herapath's 4th postulatum, wherein he assumes that " heat arises from an intestine motion of atoms, or particles." We have no direct evidence of the existence of such intestine motion, nor of an analogous motion producing like effects. The assumption of such a principle resembles contriving an empirical formula which within certain limits approximates to the truth, but no general conclusion can be established by it, and every particular one must be proved by experience before it can be relied upon.

Besides, the vibratory motion of the parts of bodies has not been shown to be a consequence of proximity to a hot body by any established mechanical principle. Of the truth of this assertion I hope he will be satisfied when some of his propositions have been examined.

Mr. Herapath assumes that matter is *inert* in his first postulatum; we have not, in my opinion, any grounds for this assumption; matter is never found inert in nature, unless the forces acting upon it be exactly balanced.

The repulsive force of the particles of gases, is one of these principles which has been assumed without strict examination; the elastic force of a gas seems to be better explained by its attraction for heat, than by the anomalous doctrine of variation from attraction to repulsion. If we consider heat to be a material fluid, this and many other phænomena are easily explained.

I only

I only notice this, because Mr. Herapath seems to consider the doctrine of repulsion a necessary part of the received theory*.

I come now to examine the propositions. In the first prop. I grant that the *duration* of the shock or stroke of perfectly hard bodies is independent of their initial velocities. If *smartness* be used as synonymous with duration, I assent to the whole prop., but not otherwise.

By the Cor. I understand that the intensity of the shock is as the momentum of the striking body.

I object to the second prop. because it requires us to assume the existence of an object absolutely immoveable: such a thing does not exist, and it is not necessary to determine the laws of collision. But in preference to refuting particular parts of Mr. Herapath's theory of the collision, I will briefly lay down what I conceive to be the true theory of the collision of hard bodies.

If a body A moving with a momentum M have all its motion destroyed by the resistance of another body B; then the momentum *m* of the resisting body B, must be equal to M.; that is, in the equilibrium of moving bodies $M = m$. This Mr. Herapath will grant; and therefore, when A strikes B, B being at rest, the reaction *m* of B, necessary to destroy the motion of A, will be equal to M; consequently B will move with the momentum M, and A remain at rest.

If the bodies meet with equal momenta, the whole motion will be destroyed, and the bodies will remain at rest.

The intensities of the strokes must be equal in the two cases, all the difference being that motion is produced by the reaction in one case, and it is destroyed in the other by the same reaction†.

Mr. Herapath will also see that the momentum communicated to a hard fixed plane, at the moment of contact, will be equal to that of the impinging body; for it is impossible to destroy momentum by mass alone, or by fixing.

If two hard bodies moving in the same direction with different momenta, so that the body having the greater momentum strikes the other, the sum of the momenta before and after the stroke will be the same, but an exchange will take place; for after the stroke, the striking body will move with the momentum of the body struck.

If the body struck be at rest before the stroke, the striking body will be at rest after it, as we have seen before.

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* Annals of Philosophy for April 1821, p. 281.

† By examining the simple case when the velocities are nothing; that is, when the opposing forces are pressures, we find wherein the mistake is made. The intensity of a pressure cannot be doubled by the mode of opposing it. See Newton's 3d Law of Motion.

If two hard bodies move in opposite directions, upon the same line, with different momenta, the momentum after the stroke will be equal to the difference of the momenta before the stroke. The body which had the greatest momentum before the stroke will be at rest after it, and the other body will move with a momentum equal to the difference of the momenta before the stroke.

If the momenta be equal, both bodies will remain at rest after the stroke, as stated before.

The whole doctrine of the collision of perfectly hard bodies turns upon this simple principle, viz. That the momentum before, and the momentum after the stroke, is the same when estimated in one and the same direction.

These conclusions being most of them different from Mr. Herapath's, and his first principles not supported by experience, I consider the principal support of his laborious superstructure to be removed; further it is not necessary to proceed.

I may also remark, that there is a much more simple and consistent manner of accounting for the greater part of the phænomena he has attempted to explain.

I am, sir,
Your most obedient servant,
THOMAS TREDGOLD.

No. 2, Grove Terrace, Lisson Grove,
Aug. 13, 1821.

XXX. *Application of the Calculation of Probabilities to the geodesic Operations of the Meridian of France. By Count* DE LAPLACE.*

THE part of the meridian which extends from Perpignan to Formentera rests on the base measured near Perpignan. Its length is about 460 thousand metres, and it is joined to the base by a chain of six-and-twenty triangles. It may be feared that so great a length, not being verified by measuring a second base near its other extremity, may be liable to a sensible error arising from the errors of the 26 triangles employed to measure it. It is therefore interesting to determine the probability that this error does not exceed 40 or 50 metres. M. Damoiseau, a lieutenant-colonel of artillery, who has just gained the prize offered by the Academy of Turin, on the return of the comet of 1759, has readily, at my request, applied to this part of the meridian the formulæ which I have given for this purpose in the second Supplement to my Analytical Theory of Probabilities. He has found that setting off from the latitude of the signal of Bugarach, a few minutes further north than Perpignan, and continuing to

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* From the *Connaissance des Tems* for 1822, page 346.

Formentera, including an arc of the meridian of about 466000 metres, the probability of an error s is proportional to the exponential.

$$c^{-\frac{3n.s^2}{4\theta^2.40350,000}}$$

c is the number which has unity for its hyperbolic logarithm, n is the number of triangles employed; θ^2 is the sum of the squares of the observed errors in the sum of the three angles of every triangle; lastly, s is the error of the total arc, the base of Perpignan being reckoned unity. Here n is equal to 26. By taking the sexagesimal second for the unity of angle, we have

$$\theta^2 = 118,178.$$

But the number of triangles employed being only 26, it is preferable to determine by a greater number of triangles the constant θ^2, which depends on the unknown law of the errors of the partial observations. To do this, use has been made of the 107 triangles employed to measure the meridian from Dunkirk to Formentera. The whole of the errors of the observed sums of the three angles of every triangle is, by taking them all positively, 173,82: the sum of the squares of these errors is 115,217. By multiplying this sum by $\frac{26}{107}$, we shall have for the value of θ^2

$$\theta^2 = 108,134.$$

This value differing but little from the preceding, ought to be employed in preference. It must be reduced into parts of the radius of the circle, by dividing it by the square of the number of sexagesimal second which this radius contains; then the preceding exponential becomes

$$c^{-(689,797)^2.s^2};$$

so that the base of Perpignan being taken for unity $(689,797)^2$ is what I name *the weight* of the result, or of the arc measured from the signal of Bugarach to Formentera. This base is $11706^m,40$; hence it has been concluded that the following fractions, which approach unity very nearly, are the respective probabilities that the errors of the arc in question are comprehended within the limits $\pm 60^m$, $\pm 50^m$, $\pm 40^m$.

$$\frac{1743695}{1743696};\quad \frac{32315}{32316};\quad \frac{1164}{1165}.$$

No reasonable doubt therefore can be entertained respecting the exactitude of the measured arc. The limits between which it may be wagered one to one that the error falls, are $\pm 8^m,0937$.

If a base of verification, equal to the Perpignan base, was measured on the coast of Spain, and joined by two triangles to the chain of triangles of the meridian; it is found by calculation, that one to one may be wagered that the difference between the measure of this base, and its value deduced from the Perpignan base, would not exceed one-third of a metre. This is nearly the difference between the measure of the Perpignan base and its value deduced from the base of Melun. It appears in the quoted Supplement, that the angles having been measured by means of a repeating circle, we may suppose the probability of an error x in the observed sum of the three angles of every triangle, proportional to the exponential c^{-kx^2}, k being a constant quantity; whence it follows that the probability of that error is

$$\frac{dx.\sqrt{k}.c^{-kx^2}}{\sqrt{\pi}}$$

π denoting the ratio of the circumference to the diameter.

By multiplying it by x, taking the integral from x null to x infinite, and doubling this integral, we shall evidently have the mean error, by taking the negative errors positively. This mean error being then denoted by δ, we shall have

$$\delta = \frac{1}{\sqrt{k\pi}}.$$

We shall have the mean value of the square of these errors, by multiplying by x^2 the preceding differential, and integrating it from $x = -\infty$, to x infinite; calling then ε that value, we shall have

$$\varepsilon = \frac{1}{2k}.$$

Hence we deduce $$\varepsilon = \frac{\delta^2.\pi}{2}.$$

Thus δ may be obtained by means of the errors, taken all plus, of the sum observed of the angles of every triangle. In the 107 triangles of the meridian, this sum is by what precedes, 173,82; we may consequently take for δ, $\frac{173,82}{107}$; which gives for $26.\varepsilon$, or for θ^2

$$\theta^2 = \frac{26.\pi}{2}.\left(\frac{173,82}{107}\right)^2 = 107,78.$$

This differs very little from the value 108,131 given by the sum of the squares of the errors of the observed sum of the angles of every one of the 107 triangles. This agreement is remarkable.

By supposing the angle of intersection of the Perpignan base, with the meridian which passes through one of the extremities of this base, well determined; we should have exactly the angle of intersection of the meridian with the last side of the chain of triangles which unite this base to the isle of Formentera, if the earth was a spheroid of revolution, and if the angles of the triangles

angles were measured exactly. The error arising from this second cause, in the last angle of intersection, is by the formulæ of the second Supplement before mentioned, proportional to the exponential c^{-r^2}, in expressing this error by $\frac{2}{3}\theta r$, which in the present case becomes 6″,8997. r. Hence it follows that the limits, within which it may be wagered one to one that the error falls, are $\pm$3″,2908. If the azimuthal observations were made with very great precision, the probability that they indicate an ellipticity in the terrestrial parallels might be determined by this formula.

The relative accuracy of the instruments made use of in geodesic operations, may be appreciated, by the value of ε′ deduced from a great number of triangles. This value, found from the 107 triangles of the meridian, is $\frac{115,217}{107}$. The same value deduced from 43 triangles employed by Lacondamine, in his measurement of 3 degrees of the equator, is $\frac{1718}{43}$; or nearly ten times greater than the preceding value. The errors equally probable, relative to the instruments employed in these two operations, are proportional to the square roots of the values of ε′. Hence it follows that the limits $\pm 8^{m},0937$, between which we have just seen that it is equally probable that the error of the arc measured from Perpignan to Formentera falls, would have been $\pm 25^{m},022$ with the instruments employed by Lacondamine. These limits would have exceeded $\pm$ 40 metres, with the instruments used by La Caille and Cassini in their measurement of the meridian. Thus it is obvious how advantageous the introduction of the repeating circle has been in geodesic operations.

A. M.

⁂ I shall feel obliged to any of your correspondents who will communicate the relative accuracy of philosophical instruments, particularly those used for observing angles, accompanying the statement with a description of the instruments sufficient to ascertain the kind to which that statement will apply.——Why is the repeating circle, which is the favourite of our scientific neighbours, so little used, and by many held cheap in England? ——What magnifying power in telescopes is sufficient for a given fineness of division in theodolites, sextants, and circles?—TRANSLATOR.

XXXI. *On promoting the early Puberty of Apple and Pear Trees when raised from Seed.* By J. WILLIAMS, *Esq.*[*]

MANY persons inclined to become experimentalists in raising fruit-trees from seed, with a view of obtaining new, improved, and more hardy varieties, have been deterred from the attempt by the great length of time requisite for ascertaining the result of their industry; for the apple-tree, when raised in the common way from the kernel, rarely affords its first blossom before it is eight or ten years old, and the pear-tree requires even a longer period, twelve or fifteen summers often elapsing before the leaves of seedling-trees are capable of forming their first blossom-buds. In November and December, 1809, I sowed the kernels of several ripe pears, in separate pots, and placed them in a green-house during the winter. They began to vegetate in the following month of February, and in March the pots were removed into my grapery, where they remained till after Midsummer. The plants were then carefully removed into a seed-bed, and planted in rows, about fourteen inches apart, where they remained till the autumn of 1811, when they were again transplanted into a nursery, at distances of six feet. Every succeeding winter I pruned away all small trifling lateral shoots, leaving the stronger laterals at their full length to the bottom of the plants, and made such a general disposition of the branches, as that the leaves of the upper shoots might not shade those situated underneath; every leaf, therefore, was thus rendered an efficient organ, by its full exposure to the light. At the height of about six feet, I had the satisfaction to observe, that the branches ceased to produce thorns, and the leaves began to assume a more cultivated character. Several of these trees afforded blossoms and fruit last year. One seedling Siberian variety of the apple, thus treated, yielded fruit at four years old, and many more at the age of five and six years.

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[*] From New Monthly Magazine, vol. iii. No. 8.

XXXII. *Contribution to the History of Electricity.* *By* A CORRESPONDENT.

To Dr. Tilloch.

SIR,—I WAS much pleased to meet the other day, in an old Scotch Magazine, with the following interesting and instructive account of some discoveries in electricity, communicated to the celebrated Professor Maclaurin by a friend in Germany, at a time when that science was almost unknown. The circumstances are new to me; and should they be equally so to you, you will probably give them a place in your valuable Magazine.

Edinburgh Aug. [illegible] 1821 Yours, C.

' When Mr. Maclaurin was professor of natural philosophy at Edinburgh, he taught in private to a select number of his students the higher parts of philosophy, which he could not undertake in his public lectures. The students who attended this second class, were generally persons of better parts and more inquisitive minds than ordinary; and being for the most part of a riper age than many of the first class, he could with propriety lay aside the dignity of the professor before them, and assume the more engaging character of the friend; and that they might be induced the more effectually to lay aside all unnecessary restraint, he used to communicate to them any new discoveries which might be made in any branch of science, and tell them frankly, without any species of reserve, the doubts and difficulties that might occur to him upon any subject. As this was his constant practice, they were not much surprised at his acquainting them one evening, that he had just received a letter from a friend of his in Holland or Germany (I cannot be positive which of these), containing, as he said, discoveries in natural philosophy, which were of such an extraordinary and whimsical nature, that he could give no manner of credit to them, and that the only conclusion he could draw from the letter of his worthy friend was, that his judgment was certainly failing, and that he had communicated the reveries of an infected imagination as discoveries in science; that on this account it had given him great uneasiness, as he supposed he had lost for ever a friend of sound knowledge, from whom he had reaped much solid instruction; and concluded with some moral reflections on the instability of all human attainments, however dazzling it might be, seeing that it might be so suddenly snatched away from them. He then produced the letter, and read it to them.

The principal contents of this letter were, that lately in the neighbourhood of the place where the writer lived, it had been discovered, that by turning a glass globe quickly round upon its axis, and at the same time rubbing it upon certain substances, it was heard to crackle, and seen to emit sparks of fire; that if any person touched it at that time, he suffered a violent shock, and seemed to have received a violent blow upon the wrists; that if any number of persons were joined together and one of them touched the globe, all of them were affected with the same violent sensations at the same time: and fire was seen to break forth where they touched each other at the same instant; but that if these persons in place of standing upon the ground stood upon certain other substances, they felt no shock at all when they touched the ball; but that if another person standing upon the ground touched them at the place where they were touched, they felt a sharp prickling pain, and fire was seen to issue from the part; that if one attempted to kiss another when standing in this manner, they were suddenly repelled from each other by an irresistible power which forced them asunder, and fire was seen to pass between the two; with many other things to the same purpose.

After having read the letter, all the students agreed that the phænomena appeared to them so very ridiculous, that they could not be induced to pay any regard to them. However, Mr. Maclaurin added, that although he was firmly persuaded that all these things were chimeras formed by the imagination, instead of facts as his friend affirmed; yet that, as the phænomena of nature were sometimes very extraordinary, and as he had on every former occasion found his friend a very sober sensible man, not ready to be misled by false appearances, he would not reject as a falsehood any thing that he had affirmed, till he had given it a fair trial; and as he had described in a very particular manner the apparatus necessary for producing these wonderful effects, he would cause one of the machines to be made in a short time, and repeat the experiment which he described. This he accordingly did. But how great was his surprise to find that upon trial all the experiments turned out exactly as they had been described! He immediately called together his students; reminded them of his former incredulity—repeated the experiments before them, and showed them how much he had been mistaken, and what injury he had done his worthy friend; with the utmost ingenuousness acknowledged his error; and warned all those who heard him to profit by this lesson which his example afforded them, and never to reckon any thing which was delivered, as a new discovery, impossible, however improbable it might appear, till they had given it the fairest trial; to preserve their minds ever open to conviction; and without the smallest hesitation readily to give up any error into which at any time they might accidentally have fallen; seeing that human reason is but weak and fallible at the best, and ought ever to be corrected by experience and accurate observation.

But he did not rest here. He went to his public class, which had till that time heard nothing of it. He told these younger students the discoveries that his friend had communicated to him; of the manner in which he had talked of him to his private class; of the experiments which he had made; of the injustice that he had done his friend; and of the error which he himself had fallen into about these experiments; talked of all these before them without the smallest palliation or glossing of any kind to conceal his own mistake, and inculcated to them the same lesson that he had given his former class.

XXXIII. *Observations on Sir* Everard Home's *Paper on the black* Rete mucosum *of the Negro*[*].

Sir Everard Home, it appears, has delivered a lecture to the Royal Society, in which he endeavours to prove, by experiment, that the *rete mucosum* of Negroes is a provision of nature against the scorching effects of the sun's rays. This, I presume, is the Croonian Lecture, which is a lecture delivered annually to the Royal Society, in pursuance of the will of a Dr. Croone, who left a sum of money to that Society, upon condition that a yearly lecture should be delivered upon muscular motion. Of late years, when that subject began to be exhausted, the lecturer has very properly been allowed to choose any anatomical subject. The task has almost always fallen upon Sir Everard Home, one of the few remaining *stars* of the English Royal Society. As it is well known that he delivers these lectures chiefly to keep up the credit of the Society, it would be hardly fair to criticise them with much severity.

Nevertheless, his ideas are so obviously wrong, and his experiments so completely inadequate, that they disgrace the Transactions of the Royal Society, and will be greatly ridiculed at Paris, and indeed everywhere by enlightened men. Professor Roux came over to London to visit the English schools; and when he returned home, he ridiculed certain things, in his public lectures, most unmercifully. But he never had so fair a theme as this.

First: As a provision against the rays of the sun, black is the very worst colour that could possibly be chosen.—What should we think of the man, who, to defend his bare poll against the scorching rays of the sun, put on a black hat instead of a white one?—Unless nature were an idiot, she certainly would have preferred white.—Sir Everard *proves* his point in the most absurd possible manner. He interposes a piece of black crape between the skin and the concentred rays of the sun; and then, because the ardour of the rays is blunted, he maintains that the *rete mucosum* of Negroes is for this purpose! The texture of the crape is the true defence, and I will never believe but that white crape would answer much better.

Secondly: Why should nature be so partial to black men? The ancient Egyptians, the ancient Hindoos, and Charibbs, all lived within the tropics; the former were white, unless in those parts exposed to the sun; the latter were red. At this day the Coquin Chinese are yellow, and yet they reside in a very hot climate. It is true, the banks of the Senegal and Gambia are hotter still: but there are black men in New Holland, and very dark-coloured skins near the north pole.

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[*] From the Newcastle Magazine.

Thirdly: There is no such thing as a pigment in the *rete mucosum* at all. In the eye, indeed, there is a pigment. There was once a Frenchman who pretended to demonstrate one in Edinburgh; but neither Professor Monro nor any person could discover it, but only the Frenchman himself. It is quite impossible to separate the *rete mucosum* from the *cutis vera* or under skin, otherwise than by an arbitrary separation. The colour of the skin depends not upon any pigment, but upon its texture; the texture of that of the Negro is thicker, but coarser wove. This would be a better preventive against the sun's rays than any pigment. But the truth is, the whole idea is ridiculous; and it was decided as long ago as the days of Buffon, that it is the obtuseness of the nervous system of the Negro which renders him callous to the most scorching heat.

The College of Surgeons boast that they never read, but make experiments only; and it may be indeed said that they know how to use their hands better than their heads. There is such a thing as drawing conclusions from experiments which the experiments do not warrant; and the above is an instance of how little use men's hands are, unless there be a head to guide them and reason upon their experiments.

XXXIV. *Notices respecting New Books.*

Recent Publications.

BAYLEY'S History and Antiquities of the Tower of London. Part I. 4to. 3*l.* 13*s.* 6*d.*

One thousand Experiments in Chemistry, accompanied by Practical Observations, &c. By Colin M'Kenzie. 8vo. 1*l.* 1*s.*

Robertson's Colloquia Chemica, &c. &c. 18mo. 6*s.*

F. Accum's Culinary Chemistry. 12mo. 9*s.* 6*d.*

Scientific Amusements in Philosophy and Mathematics. By W. Enfield, M.A. 12mo. 3*s.* 6*d.*

A Practical Treatise on the Hydrocephalus Acutus, or Water Inflammation in the Head. By L. Golis, of Vienna. Translated by Robert Gooch, M.D. 8vo. 8*s.* boards.

A Treatise on Cataract. By P. C. De La Garde, &c. 8vo. 8*s.* boards.

The Theory of the Plague, as it has lately appeared in the islands of Malta, Goza, Corfu, &c. &c. By J. D. Tully, Esq. Surgeon to the Forces. 8vo. 10*s.* 6*d.*

Observations on certain Affections of the Head, commonly called Head-Aches, with a view to their more complete Elucidation, Prevention, and Cure. By James Farmer, Dublin. 18mo. 2*s*.

An Analysis of the Natural Classifications of Mammalia, for the Use of Students and Travellers. By T. Edward Bowdich, Esq. 8vo.

An Introduction to the Ornithology of Cuvier, for the Use of Students and Travellers. By T. Edward Bowdich.

Zoological Researches in the Island of Java, &c. &c. with Figures of Native Quadrupeds and Birds. By Thomas Horsfield, M.D. F.L.S. Number I. 4to. 1*l*. 1*s*.

General and Particular Descriptions of the Vertebrated Animals, arranged conformably to the Modern Discoveries and Improvements in Zoology. By Edward Griffith. Part I. Monkeys and Lemurs. Imperial 8vo. With coloured plates, after drawings from Nature. 1*l*. 5*s*. boards.

Sprengel's Philosophy of Plants. 8vo. 15*s*.

A Grammar of the Sanscrit Language, in one volume, 8vo, on a new plan. By the Rev. William Yates. Dedicated, by permission, to the Most Noble the Marquis of Hastings. Calcutta. price 2*l*. 10*s*., fine 4*l*.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE following papers have been read at the Meetings of the Society since our last Report.

May 10, 1821.—Some Remarks on Meteorology, by Luke Howard, Esq.

A Calculation of some Observations of the Solar Eclipse of the 7th of September, 1820, by Mr. Charles Rumker, communicated by Dr. Thomas Young, For. Sec. R.S.

21. On the Anatomy of certain parts of the globe of the Eye, by Arthur Jacob, M.D. Communicated by Dr. James Macartney.

31. Experiments on Temperature, with a view to determine the Ratio of Temperature, and the Point of absolute Cold, by John Herapath, Esq. Communicated by Davies Gilbert, Esq. Tr.R.S.

June 7. An Account of the Remeasurement of the Cube Cylinder and Sphere, used by the late Sir George Shuckburgh Evelyn, in his Inquiries respecting a Standard of Weights and Measures, by Captain Henry Kater.

21. An Account of Observations made at the Observatory of Trinity College, Dublin, since 1818, for investigating the parallax and aberration of the Fixed Stars, and effects of Lunar Nutation, by the Rev. John Brinkley, D.D.

28. On the effects produced in the rates of Chronometers, by the proximity of masses of iron, by Peter Barlow, Esq. Communicated by John Barrow, Esq.

July 5. Some positions respecting the effect of the Voltaic Battery in obviating the effects of the division of the 8th pair of nerves, by A.P. Wilson Philip, M.D. Communicated by B. C. Brodie, Esq.

On the Magnetic Phænomena produced by Electricity, and their relation to Heat occasioned by the same agent. By Sir Humphry Davy, Bart. F.R.S.

12. An investigation of some Theorems relating to the Theory of the Earth, and the principle of Equilibrium in Fluids, by M. Hoene Wronski. Communicated by John Pond, Esq. Astr. Roy.

On the peculiarities that distinguish the Manatee or Dugong of the West, from that of the East Indies, by Sir Everard Home, Bart. V.P.R.S.

On a new compound of Chlorine and Carbon, by Richard Phillips and Michael Faraday, Esq. Communicated by the President.

On the Nerves, giving an account of some experiments on their structure and functions, which lead to a new arrangement of the System, by Charles Bell, Esq. Communicated by the President.

EDINBURGH SCHOOL OF ARTS.

A school of arts has been established in Edinburgh, for the instruction of mechanics in such branches of science as are of practical application in their several trades. Lectures on practical mechanics and practical chemistry will be delivered twice a week, during the winter season. A library containing books on popular and practical science has already been established. The institution is conducted under the direction of a committee of fourteen, having a clerk and librarian.

FRENCH ASIATIC SOCIETY.

A number of learned men have united to form, at Paris, an Asiatic Society, the object of which is to encourage, in France, the study of the principal languages of Asia. It is their intention to procure oriental MSS.; to circulate them either by means of printing or lithography; to have extracts or translations made of them; and to join in the publication of grammars and dictionaries. This new institution will correspond with other societies which devote themselves to the same object, and with learned men who apply to the study of the oriental languages.—25 francs per annum is to be the subscription; and many learned men are enrolled.

XXXVI. *Intelligence and Miscellaneous Articles.*

MEXICAN FLORA.

At the last anniversary sitting of the Helvetic Society of Natural Science, M. de Candolle presented to the Society a *Flora of Mexico,* consisting of 1740 leaves, and forming 13 large folio volumes. The following account of this work is given in the *Morgenblatt,* published at Stutgard:—MM. Sesse, Mocino and Cervantes had travelled over New Spain, with the view of collecting a Mexican Flora. They made a drawing of each plant on the spot where they found it. M. Mocino had returned to Madrid, in order to have the drawings thus obtained engraved, when the first troubles in Spain obliged him to seek refuge with his Flora at Montpelier. M. de Candolle, who was then at Montpelier, became acquainted with M. Mocino, and assisted him for eighteen months in arranging systematically his numerous collection. M. de Candolle afterwards went from Montpelier to Geneva, and M. Mocino gave him the Flora along with him, that he might one day send it forth to the world. The new aspect of affairs in Spain having induced M. Mocino, however, to return to his native country, he wrote lately to M. de Candolle, requesting to have the Flora back. The French naturalist, unwilling to run the chance of losing all trace of so valuable a treasure, immediately requested some friends to copy part of the rarest drawings for him. No sooner was this known in Geneva, than numbers of persons of both sexes offered their services; and in the end every person capable of managing a crayon or a pencil was occupied with the Mexican Flora. They worked with such zeal, the ladies especially, that in the short space of eight days there was not a single drawing remaining to copy.

NEW SHETLAND.

Several vessels have been to this newly discovered southern land, and have returned with good cargoes of very fine seal-skins. The John of London, Captain Walker, brought home 12,000. The extent of country explored from east to west, from Clarence Isle to Smith's Cape, is from 54 to 61 degrees west longitude, and from 61 to 64 degrees south latitude, and the land seen to the southward, as far as the eye can reach. The country already explored consists of numerous islands, without a vestige of vegetation. A species of moss only is found upon the rocks near the shore; eternal snows covering the more remote parts, which are mountainous. Although nature, in those regions, assumes the most sterile and forbidding features, the thermometer was at no time below the freezing point; but the melting snows near the shore so completely saturate the soil as to check all vegetation. A species of coal was found in abundance, which burnt very well, a specimen of which we have seen, thus affording the means, if wanted, of replenishing the fuel. The rise and fall of the tide is about twelve feet. Shrimps and penguins are beyond all conception numerous. The islands, headlands, &c. have been named, and the observations ascertaining the latitude and longitude, from repeated experiments, found true; so that we may soon hope to see a correct chart, from the surveys which have been taken, on the arrival of Captain Smith, in the Blythe, who is shortly expected.—Part of an anchor-stock, evidently Spanish, being bolted with copper, and bearing certain marks, was found on shore, and is presumed to be the only vestige now remaining of a 74-gun ship of that nation, which sailed from Spain, bound to Lima, about eighteen months or two years ago, and has not since been heard of.

The following are the latitudes and longitudes of the newly discovered country towards the South Pole:—

	South		West	
Start Point	62°	42′	61°	28′
Cape Sherriff	62	26	60	54
Desolation Island	62	27	60	35
Smith's Island	—	—	—	—
Cape Melville	62	1	57	44
Martin's Head	62	12	58	20
Penguin Island, South End	62	6	58	6
Bridgman's Island	—	—	—	—
Tower Island	63	30	60	30
Hope Island	63	5	57	4
Cape Bowles	61	19	54	10
O'Brien's Island	—	—	—	—
Seal Island and Reef	61	1	55	33
Cape Valentine	61	3	54	48
Cornwallis Island	60	0	54	36
Lloyd's Promontory. Clarence's Island, North End	61	2	54	10
Ridley's Island	61	5	58	23
Falcon Island	62	18	59	56

SUBMERSION OF THE VILLAGE OF STRON, IN BOHEMIA.

As reported in a Letter from M. Winkler.

The village of Stron, in the estate of Fermian, in Bohemia, was situated on a declivity, in the N.E. of the valley of Eger, about a league above Saatz, partly near the river, and partly in a gorge that descended towards the Eger. On a hill that forms a border to this gorge, were the church and parsonage-house, and the village descended along the gorge parallel to the Eger, towards the N.W. This hill contains beds of an earthy pit-coal that spread through the country, and are covered with strata of sand and alluvion. The Eger flows at the distance of about 200 toises from Stron. Previous to the accident, it formed a bay alongside of Stron, edged with hills of moving sand, not very lofty, but steep. On the higher part of the declivity were a number of springs, that were quickly lost in the sands.

These springs have proved the cause of a calamity, which, in these countries, where glaciers and earthquakes are unknown, may be deemed unique in its kind. The water of the springs has gradually perforated large subterranean cavities in the strata of sand, so that, at length, the whole surface of the soil, with the church, the houses and the gardens, rested only on some detached columns of sand that were daily diminishing. Whether subterranean combustions of pit-coal may not have co-operated, is a point hitherto undecided.

For a length of time the earth had been sinking in different places. Crevices appeared in the walls of the buildings; the doors would no longer shut, and some weeks ago a great noise was heard in the middle of the night. The people are roused from their sleep; a singular movement of the earth advancing forward, and at the same time sinking, is observed. The inhabitants flee, remove their cattle, &c. and at some distance from the village wait for the morning. Its appearance displays an image of destruction; half of the village had disappeared; where no houses had ever been, roofs and chimnies were seen rising from the ground. The hill, the church, and the parsonage, were no longer to be found; and at some distance appeared a chaos of parcels of earth intermixed with ruins and crevices.

The church is 80 feet below the site it formerly occupied; it is divided into two, half of it buried in ruins. Here lies a steeple overthrown, and there a confused medley of statues, images of saints, stables, &c. The river is thrown out of its channel, and where it formed a bay, there is now an accumulation of earth. The churchyard is thrown into a shapeless heap, and the whole territory bears another aspect. In different patches are seen layers of a fat earth, over which the sand has glided. It seems that the Eger must have crumbled the props on which the hill stood, as they had ever an inclination towards the river.

A number of things have been fortunately preserved, and, with the exception of some cattle, no lives were lost. Fifteen houses are yet standing; but the soil is insecure, and the downfall will probably be universal.

I was at a loss, at first, to recognise the country; and from the inhabitants I could only learn that they had been disturbed by a tremendous crash, and that they sought refuge by flight. The people were rich; their loss, in point of furniture, is not so considerable as in the superficies of the soil.

The village is now a sort of central spot for pilgrimage to the whole of Bohemia; the curious flock hither from every quarter, to explore the effects of this phænomenon. It is impossible to form a just idea of it without inspection.

EGYPT.

The Prussian State Gazette contains the following extract of a letter from the Prussian Major-general Baron Minutoli, dated Cairo, April 13, 1821:

" I am, thank God, in good health, though we have had, for some days, a burning *chamsin*, which threatens general suffocation, and gives to the coolest apartments a temperature of 28° or 30° by Reaumur's thermometer. This dangerous wind has a very bad effect, and promotes the eruption of the plague, which, for these three months past, has at times prevailed in Alexandria. Here only two persons have been attacked by it, but every body is apprehensive of the further spreading of this scourge of the East. I think like the inhabitants, *Alla Hrnii* (as God will), and go every day into the city, but avoid, as much as possible, coming in contact with the Arabs, who throng the streets. To-morrow I shall take leave of the pacha, who is at his country house Schoubra; and shall set out in a few days for Jerusalem, by way of Damietta and Jaffa.

" I have had the drawing of my pyramid finished, and shall publish it, with my Journal, in two plates. The internal construction is very remarkable, and may probably throw much light on these most interesting monuments. A few days ago, my workmen found the gilded skull, the feet, and the hands of a mummy; and I am inclined to infer, that these remains, the only ones of their kind hitherto found, are those of the king entombed in the pyramid. I understand that other interesting objects were discovered: but the rapacious Arabs sold them to other persons; which I regret the more, as they might have led to some knowledge of the purpose of the pyramid. From the ground-plan, I am disposed to conclude, that its *Rumisti Kabiren*, which are not yet all opened, extend a great way, and lead to sepulchres or sanctuaries which lie beyond the pyramid. This raised the idea, that the entrance to the celebrated labyrinth may, perhaps, be found in the neighbouring pyramids. In the inspection of my pyramid I might easily be buried alive. But something must be ventured by him who will make conquests even in the domain of science. My pyramid has seven breaks instead of six, and is not a regular square. A very handsome sarcophagus, with hiero-

glyphics,

gliphics, was lately taken from the catacomb which I caused to be opened. A second, in admirable preservation, together with the beautiful catacomb richly adorned with hieroglyphics, has been sold by mistake to other friends of art. How much might still be done here with sufficient means and time!

THE UNICORN.

Mr. Campbell (the missionary) has kindly favoured us with the following description of the head of a very singular animal which he has just brought from the interior of Africa. We also have had an opportunity of seeing it, and fully agree with Mr. Campbell, that the animal itself must have answered the description of the *Reem* or *Unicorn*, which is frequently mentioned in Scripture.

"The animal," says Mr. Campbell, "was killed by my Hottentots, in the Mashow country, near the city of Mashow, about two hundred miles N. E. of New Lattakoo, to westward of Delagoa Bay. My Hottentots never having seen or heard of an animal with *one* horn of so great a length, cut off its head, and brought it bleeding to me upon the back of an ox. From its great weight, and being about twelve hundred miles from the Cape of Good Hope, I was obliged to reduce it by cutting off the under-jaw. The Hottentots cut up the rest of the animal for food, which, with the help of the natives, they brought on the backs of oxen to Mashow.

"The horn, which is nearly black, is exactly three feet long, projecting from the forehead about nine or ten inches above the nose. From the nose to the ears measured three feet. There is a small horny projection of about eight inches immediately behind the great horn, designed for keeping fast or steady whatever is penetrated by the great horn. There is neither hair nor wool on the skin, which is the colour of brown snuff.

"The animal was well known to the natives. It is a species of the rhinoceros; but if I may judge of its bulk from the size of its head, it must have been much larger than any of the seven rhinoceroses which my party shot, one of which measured eleven feet from the tip of the nose to the root of the tail.

"The skull and horn excited great curiosity at the Cape. Most were of opinion that it was all we should have for the unicorn.

"An animal, the size of a horse, which the fancied unicorn is supposed to be, would not answer the description of the unicorn given by Job, chap. 39, verse 9 *et seq.*, but in every part of that description this animal exactly answers to it."

(Signed) "JOHN CAMPBELL."

Pliny's description of the unicorn is a sort of medium between Mr. Campbell's account, and the animal depicted on the royal coat of arms. It is as follows:—"Asperrimam esse feram, reliquo corpore similem equo, capite cervo, pedibus elephanti, caudâ apro, mugitu gravi, uno cornu nigro mediâ fronte cubitorum duum eminente."

Our readers are aware that measures have been taken to obtain a complete specimen of the animal supposed to be the unicorn, which is said to exist in considerable numbers in Thibet. The description which has hitherto been furnished us rests entirely on the evidence of natives; but as it differs in several essential points from Mr. Campbell's account of the African unicorn, the scientific world will be anxious to compare the specimens as soon as they are enabled to do so. Mr. Campbell's demonstration is the best as yet, and will probably never be excelled. (*Asiatic Journal.*)

LIZARD IMBEDDED IN STONE.

In our last Number we mentioned a curious instance which had occurred at Auchtertool in Scotland, of a lizard being found imbedded in a large block of stone. The phænomenon is thus ingeniously attempted to be accounted for in a work which Mr. Welch, of Stonehouse, Devon, has in the press, entitled *Religiosa Philosophia.*

"This phænomenon," says the author, "is a further testimony in favour of the principle of the present work; and if the author may be allowed to venture an opinion how the lizard became imbedded, and by what means it was preserved in this solid mass of stone, he offers the following:

"Nature, in all her operations, evinces a peculiar tenacity in preserving the principle of life, both in the vegetable and animal kingdoms: hence it is that the seeds of many plants preserve the germ, or vivifying principle, through a series of years; whilst the eggs of birds, situated so as to exclude them from the effects of atmospheric air, retain their fecundity for a considerable length of time. Having thus premised, I proceed to state, that the ova or spawn* of a lizard was, either by means of water, or some other cause, conveyed into a situation where Nature was preparing this stone; that the sand, whilst forming around the ova, gradually became expanded, from a principle of life which the egg contained, and which, being surrounded by its own atmosphere, arising from native heat, tended to bring forth the animal, whilst the same cause produced a sufficient cavity to contain it when arrived to its full size. Now as the lizard, when first taken out of the stone, exhibited no signs of animation for the space of five minutes, we may fairly presume that the animal had been preserved in its entombed cavern in a state of torpor, until, by the vivifying influence of the sun, it awoke as out of sleep; whilst the air, inflating the lungs, giving circulation to the blood, and motion to the heart and limbs, caused the lizard to spring into life! Hence this illustration may, in many instances, serve to explain the interesting phænomena of frogs, toads, and other animals, having been found in the cavities of trees, or imbedded in masses of stone."

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* The egg or spawn of some species of the lizard is covered with a shell of hard calcareous substance, considerably thicker than that of the egg of a bird, consequently less brittle in its nature.

FOSSIL ELK.

One of the most perfect and beautiful specimens that has yet been found was discovered a few months ago in the Isle of Man, in digging a marl pit. This skeleton has been presented by His Grace the Duke of Athol to the museum of the University of Edinburgh.

DRUIDICAL ANTIQUITIES.

There were lately found in the neighbourhood of Belfast two antique golden crescents of a large size, made of pure gold, and weighing about 6 ounces each. It is supposed that they were used as bells by the Druids in celebrating their mysteries; and the fine tone produced by striking the cup at the ends of the crescent gives some countenance to the opinion. Near the spot where they were found are the remains of two Druidical altars.

STRUCTURE OF CRYSTALS.

Recent investigations having directed the attention of observers, in a particular manner, to the study of the optical characters of crystallized minerals, we think it may not be without use to notice a circumstance in the structure of crystals, which, if not known, or neglected, may lead into error. Many crystals, which, in a general view, appear simple, are found to be compound, when all their relations are attended to; and these, when examined optically, will present a *compound* in place of a *simple* structure. The simple structure characterizes the species of minerals, while the compound structure often distinguishes the variety, or sub-species.—*Edin. Phil. Journal.*

ANTIENT MANUSCRIPTS.

Some further interesting discoveries of lost works have been made by M. Maio, among which are several parts of the mutilated and lost books of Polybius, of Diodorus, of Dion Cassius, some fragments of Aristotle, of Ephorus, of Timeus, of Hyperides, of Demetrius of Phalaris, &c. some parts of the unknown writings of Eunapius, of Menander of Byzantium, of Priscus, and of Peter the Protector. Among the inedited works of Polybius are prologues of the lost books, and the entire conclusion of the 39th, in which the author takes a review of his history, and devotes his 40th book to chronology. The fragments of Diodorus and of Dion are numerous and most precious. Among them is a rapid recital of many of the wars of Rome; a narrative of the Civil, Punic, Social or Italic, and Macedonian wars; those of Epirus, Syria, Gaul, Spain, Portugal, and Persia. Parts of the history of the Greeks and other nations, and that of the successors of Alexander, &c. are among these. They were discovered in a MS. containing the harangues of the rhetorician Aristides, from a large collection of ancient writings, made by order of Constantinus Porphyrogenetes, of which only a small part are known to be extant. The writing appears to be of the 11th century. M. Maio has also met with an unedited Latin grammarian, who cites a number of lost writers, and a Latin rhetorician now unknown; also a Greek collection containing fragments of the lost works of Philo. He has also found writings of the Greek and Latin fathers prior to St. Jerome, with other valuable works, all of which he intends shortly to publish.

HORIZONTORIUM.

We have recently seen a curious philosophical plaything under this name, which is, we believe, published by Mr. Bancks, the mathematical-instrument-maker, in the Strand. The inventor's name is Shires, and the invention itself is an exceedingly pleasing optical illusion. This is produced by the picture of a castle, projected on a horizontal plane, whence its name is derived. The picture is laid flat on the table, with the light on the left of the spectator. In front there is a small perpendicular parchment sight, with a groove in it, to which the eye is applied; and the effect is, that the whole appears to be a solid building; the walls of the castle, the rim of a well, &c. &c. being, in every respect, like a model, instead of a coloured horizontal projection. By removing the candle to the floor, that which was a sun-light becomes a moonlight scene. The illusion is very pretty, and the thing, in its application, though not in its principles, entirely new to us.

MECHANICAL INVENTION.

An invention has been made by a young man belonging to Mauchline—Mr. Andrew Smith, of the Water of Ayr Stone Manufactory. This is an instrument for copying drawings, &c., called by the learned who have seen it an Apograph. It is so constructed, that drawings of any kind may be copied by it upon paper, copper, or any other substance capable of receiving an impression, upon a scale either extended, reduced, or the same as the original. The Arts, we understand, furnish no instance of an instrument resembling this, either in its appearance or operation, save what is called the Pentagraph, and even from this machine it differs materially. The beam in the former is suspended vertically from an universal joint, whereas the beam of the latter is supported on an horizontal plane. There is also a counterpoise added to the Apograph above the centre of motion, which relieves the hand almost entirely of the weight it would otherwise have to sustain when the beam is out of the vertical position.—*Ayr Advertiser.*

HYDROPHOBIA.

(From a French Journal)

A series of experiments have recently been made at the Veterinary School in Paris, relative to the cure of hydrophobia.

The object in view was, to confirm the efficacy of a specific imported from Italy, which, it is expected, will not only act as a preservative immediately after the bite, but will also operate as a cure even after the fatal symptoms have appeared. The result of these experiments is not yet ascertained.

LECTURES.

St. George's Medical and Chemical School.—The Courses will commence the first week of October.

1. On the Practice of Physic, with the Laws of the Animal Œconomy; by George Pearson, M. D. F. R. S. Senior Physician to St. George's Hospital, &c.
2. On Chemistry; by W. T. Brande, Professor Royal Institution, Sec. R. S., &c.
3. On Therapeutics with Materia Medica; by George Pearson, M. D. F. R. S., &c. &c.

LIST OF PATENTS FOR NEW INVENTIONS.

To Robert Dickinson, of Great Queen-street, Lincoln's-Inn-fields, for certain improvements in the construction of vessels or crafts of every description, whereby such vessels or crafts may be rendered more durable than those heretofore constructed for the purpose of navigation.—Dated 14th July 1821.—6 months allowed to enrol specification.

To Samuel Cooper, engineer, and William Miller, gentleman, both of Margate, for certain improvements on printing machines. —17th July.—6 months.

To Frederick Mighells Van Heythuysen, of Chancery-lane, for a new method of propelling small vessels or boats through water and light carriages over land.—23d July 1821.—6 months.

To David Barclay, of Broad-street, London, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for a spiral lever or rotary standard press.—26th July.—6 months.

To Thomas Barker, of Oldham, Lancaster, and John Rawlinson Harris, of Winchester Place, Southwark, hat manufacturers, for improvements in the method of clearing furs and wools used in the manufacture of hats from kemps and hairs.—26th July.—6 months.

To John Richard Barry, of the Minories, in London, for certain improvements on and additions to wheeled carriages.—26th July.—6 months.

To Samuel Bagshaw, of Newcastle-under-Lyme, for a method of forming and manufacturing vases, urns, basins, and other ornamental articles which have been heretofore usually made of stone or marble, from a combination of materials never heretofore made use of in manufacturing of such articles.—26th July.—2 months.

To John Manton, of Dover-street, Piccadilly, gunmaker, for improvement in the construction of locks to all kinds of fowling pieces and fire arms.—30th July.—2 months.

To Thomas Bennett, jun. of Bewdley, Worcestershire, builder, for certain improvements in steam engines or steam apparatus.—4th July.—6months.

To John Slater, of Birmingham, manufacturer, for certain improvements in making a kitchen range and apparatus for cooking and other purposes.—4th August.—6 months.

To William Henry Higman, of Bath, sadler and coach harness maker, for certain improvements in the construction of harness, which he conceives will afford great relief to horses in drawing carriages of various descriptions, and be of public utility.—14th August.—2 months.

To David Gordon, of Edinburgh, at present residing in the town of Stranraer, esq. for certain improvements in the construction of wheeled carriages.—14th August.—6 months.

To Jean Frederick Marquis de Chabannes, for a new method and apparatus for attracting and catching of fish.—14th August.—6 months.

To John Collinge, of Lambeth, engineer, for improvements on cast iron rollers for sugar mills by more permanently fixing them to their gudgeons.—14th August.—4 months.

To John Nichol, of West End, Middlesex, master mariner, for improved capstan windlass and hawse roller.—22nd August.—2 months.

Epping, July 12, 1821.

Sir,—The following observations made at this place on the 14th of May, the 11th of June, and 9th of July, were taken with great care at the under-mentioned times.

1821. A.M.	H. M.T.	Barom.	Ther. att.	Ther. det.	Wind.	Clouds, &c.
May 14th	8	29 018	49	46	W.	*Cirri* with flying *cumuli* and sunshine.
	9	29·018	49	47	W.	Do.
	10	29·020	49	49	W.	Do.
	11	29·022	49	51	W.	Stormy, with some dense *cumuli.*
	12	29 024	49	48	W.S.W.	*Cumuli* and *nimbi*: from the latter fell a heavy shower of hail and rain.
June 11th	8	29·538	49	48	N.N.E.	*Cirri, cumuli,* and sunshine.
	9	29·570	50	49	N.N.E.	*Cirrostrati, cumuli,* and sunshine.
	10	29 562	50	51	N.N.E.	The same modifications as before, though diminution of *cirri,* and an increase of *cumuli.*
	11	29·562	51	52	N.N.E.	Brighter; *cirri,* and flying *cumuli.*
	12	29·562	51	51	N.	Some *cirri*; a great decrease of *cumuli.*
July 9th	8	29 782	55	54	N.W.	Sunshine, with *cirrocumuli.*
	9	25·782	55	56	N.W.	Bright; some *cirrocumuli* to leeward.
	10	29·782	55	59	N.W.	Bright; some few clouds southward, with here and there nascent *cumuli.*
	11	29·782	56	61	W.N.W.	Sun; clouds much increased to windward: two currents.
	12	29·782	57	61	W.N.W.	Dark *cumulostrutus* to windward.
	P.M.					
	1	29·782	57	60·5	W.N.W.	Sunshine with thin *cumuli.*
	2	29·782	58	63	W.N.W.	Dark *cumulostratus*; no sun.
	3	29·782	58	62	W.N.W.	Sunshine with thin *cumuli.*

The wind was rather brisk during the whole of the time. It may be seen that the Barometer was stationary during the whole time of the observations of the 9th of July, and it continued so till near noon of the following day. Yours truly,

To Dr. Tilloch. Thomas Squire.

Observations on the Barometer, by W. Bagge, Esq., Lynn, Norf.

	Clock.	Barom.	Attach. Ther.	Detached.
June 11th.	8 morn^{g}.	30·03	57°	46
	9	30·04	60	46
	10	30·05	60	
	11	30·07	60	
	12	30·08	60	55
	1	30·10	60	
	2	30·11	60	55
	3	30·13	60	

Height of the cistern 29 feet 5 inches from low-water mark, spring tides.

Register kept by Dr. Burney at Gosport.

Hour.	Barom	Ther.	Wind	State of the Weather.
1821. A.M.				
Aug 13. 8h	Inches 30 10	62|65	S.W.	A turbid appearance of *cirrostratus*, and large dense *cumulostrati* with white tops, floating beneath to the eastward, so that only a little of the sky between the sun and the eastern part of the horizon could be seen. Light airs from S.W., to which point the wind has veered from S.E. within the last hour.
	30·11	|68|	S.W.	An extensive and lofty bed of *cirro-stratus*, still of a turbid aspect, low *cumuli* around the horizon, and a gentle breeze.
10	30 12	68|69' 63|	S W.	Linear *cirri* in the light blue sky to the westward, a bed of *cirrocumulus* in small, round, bright flocks in the vicinity of the sun, attenuated *cirrostratus* with apertures therein, and *cumuli*, all in regular succession downwards—these modifications of clouds had a slow motion in the direction of the wind, which was freshening.
11	30 14	70|72|	S. W.	The clouds nearly the same as at 10 o'clock, but more dense—the wind still freshening, and the sky of a deeper blue colour.
12	30 13	74 72|	S.W.	Hot sunshine at intervals through the compound clouds, which have almost overcast the sky. A steady breeze prevails, and the barometer and hygrometer slowly rise and fall.
P M 1	30 14	|74	S.W.	A completely overcast sky, followed by steady rain in the afternoon, and showers in the evening.

N.B. In consequence of a communication from John Farey, esq. sen. the height of the barometer in the above observations is not reduced to the temperature of 32°.

Bristol, July 23, 1821.

Sir,—Having mislaid my barometric observations for May and June till too late for your last Number, I now send them together with those made on the 9th inst.

And am, sir, your obedient servant,
Edward Jones.

Barometric Observations.

1821.	Barom.	Thermom. att.	Thermom. det.	Wind.	Weather.
May 14th.					
8h	29·250	50	47½	W.N.W.	Fine.
9	29·250	51½	49½	W.	Do.
10	29·250	53	51	W.	Rain.
11	29·250	53½	53½	W.	Do.
12	29·247	54½	55	W.N.W.	Fine.
June 11th.					
8	29·768	52½	50½	N.N.E.	Cloudy.
9	29·770	53	52	Do.	Do.
10	29·793	53	51	Do.	Rain.
11	29·805	54	53	Do.	Do.
12	29·810	53	52½	Do.	Do.
June 9th.					
8	29·953	59	61	W.	Fair.
9	29·951	60	64	Do.	Do.
10	29·948	60½	65	W.N.W.	Fine.
11	29·943	61	66	W.	Do.
12	29·938	62	67	Do.	Do.

Arundel, Aug 14, 1821

SIR,—I again trouble you with the following Barometric Observations made at this place on the 9th of July and 13th of August. And am, sir,

Your obedient servant,

To the Editor. G. CONSTABLE.

Hour.	Barom	Thermo att.	Thermo det	Wind.	Weather.
July 9th,	°	°	°		
8h	30·180	57·5	56·5	N by W. calm	Thin clouds
9	30·180	58·5	57·5	N. by W. do.	Sunshine with clou.
10	30·183	59·5	58·5	N. mod breeze.	Do.
11	30·183	60·5	59·0	N W. do.	Do.
12	30·180	61·0	60·0	W N W. calm.	Dense clouds.
Aug 13th, 8	30·059	60·0	59·5	N W. do.	Thin white clouds.
9	30·060	61·0	60·0	W.S.W. mod. breeze.	Flying gray clouds.
10	30·060	62·0	61·0	S.W do.	Do
11	30·060	63·0	61·5	S.W. do	Cloudy with sunshine.
12	30·060	63·5	62·0	S.W. fresh breeze.	Do.

Crumpsall, Lancashire, Aug. 15, 1821.

SIR,—I send you the observations made here, and at Manchester, on the 9th of July and the 13th of August.

Your obedient servant,

To the Editor. JOHN BLACKWALL.

CRUMPSALL.

1821 A M	Bar.	Ther att	Ther. det.	Wind.	Weather.
July 9th 8h.	29 762	56°	54°7	W. by N. brisk.	Cloudy.
9	29 760	55 5	55	W. by N. do.	Do.
10	29 760	55 5	54 5	W. do.	Do.
11	29 760	55·5	55	W. do.	Do.
12	29 760	55 5	55 5	W. by N. do.	Do.
P M. 1	29 760	56	57	W do.	Do.
Aug 13th, A M.					
8	29 580	57	55 7	S. by E. light.	Foggy, with light rain.
9	29 580	57	56 5	S. by E. do.	Cloudy.
10	29 580	58 5	59	S. do.	Do.
11	29 570	59 5	60	S. fresh.	Do.
12	29 560	60 5	61·5	S. do.	Cloudy, with faint sunshine.
P M 1	29 555	61	62 5	S. do	Cloudy

MANCHESTER.

1821 A M	Bar	Ther. att	Ther. det.	Wind.	Weather.
July 9th					
8	29 980	60°	60°	W fresh.	Cloudy.
9	29 980	60	61 5	W. do.	do.
10	29 985	60 5	62	W. do.	do.
11	29 990	60 5	62 5	W do	do.
12	29 950	60 5	62 5	W. brisk	do.
P M 1	29 960	62	61 5	W. fresh.	do.
Aug 13th, A.M.					
8	29·820	60	63	S. do.	Hazy.
9	29·815	62 5	64	S. do.	Cloudy.
10	29 805	64	65 5	S. do	do.
11	29·790	64 5	66	S do.	do
12	29 780	66	69	S do.	Gleams of sunshine.
P M 1	29 780	66 5	69·5	S. do.	Cloudy.

Leighton, Aug. 20, 1821

DEAR SIR,—I have the pleasure to send the observations made at this place by my son during my absence on the 13th instant, as below:

1821.	Barom	Ther att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·766	57½	57	S.W.	calm.	Cloudy.
9	29·766	58½	61	S.W.	moder.	Do.
10	29·767	60	65	S.S.W.	do.	Partially do.
11	29·762	60½	65½	S.S.W.	do.	Do.
12	29·756	61½	68	S.S.W.	do.	Do.
1	29·761	62	67	S.S.W.	do.	Cloudy.

The thermometers suspended near the middle of the barometrical tube were 3½° higher than the inclosed thermometer at eight and nine o'clock, and 2½° higher at twelve and one, averaging 3° above the basin.

The observations made as usual by Colonel **Beaufoy**, are as follow:

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·523	57·5	56·5	S.S.W.	moder.	Cloudy.
9	29·523	57·3	59	W. by S.	do.	Do
10	29·525	58·7	61·5	S.W.	fresh.	Fine.
11	29·522	59·	64	W.S.W.	do.	Do.
12	29·520	59·7	65	W.S.W.	do.	Do.
1	29·517	60·	65	W.S.W.	do.	Cloudy.

Colonel Beaufoy has calculated the height of Bushey above Leighton, from the July observations, to be about 247 feet. I observed with pleasure the attention shown to this subject by Dr. Burney; I am however afraid the wheel barometer cannot be relied upon, but in the absence of a better instrument may be used with some advantage if correctly compared with a good standard instrument *previous to any removal* from the situation it occupied at the time of observation.

It appears to me that Gosport is too far from London to be used as a medium of transfer of the zero, provided full reliance could be placed on the barometrical calculations; but in the present state of our knowledge of the atmosphere, it would in my opinion be unsafe to trust to that mode of fixing the zero in London, even in the shortest line that could be drawn to the coast; and as the *section of the river Thames* is a most interesting object independent of our present inquiry, it is to be hoped that nothing short of actual levelling will satisfy the public in transferring the *zero* from the coast to London.

I have received a letter in the name of Mr. Ralph Tredgold, suggesting a more extended course of observations, to which I beg to state, that some arrangements have long been in train to render the advantages of the barometrical observations more general and certain, by which any person may render available as many observations *every day* as may be convenient to himself.

I am, dear sir, yours very truly,

To Dr. Tilloch. B. **Bevan.**

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS			
July 15	full	62°	29·35	Cloudy—heavy rain A.M.
16	17	67°	29·60	Fine
17	18	68°	29·90	Ditto
18	19	70°	29·90	Ditto
19	20	76°	29·65	Ditto
20	21	75°	29·33	Ditto—rain with thunder and lightning A. M.
21	22	71·5	29·28	Ditto
22	23	72°	29·10	Ditto—stormy with rain A. M.
23	24	68°	29·14	Ditto—rain with thun. & light. P.M.
24	25	69°	29·30	Ditto
25	26	56°	29·30	Rain
26	27	63°	29·43	Cloudy
27	28	65·5	29·50	Fine—heavy rain with thunder and lightning P. M.
28	29	64·5	29·58	Cloudy
29	new	66°	29·65	Ditto
30	1	71°	29·45	Fine—rain P.M.
31	2	66°	29·50	Cloudy
Aug. 1	3	69°	29·43	Ditto—heavy shower P. M. with rainbow.
2	4	68°	29·68	Ditto
3	5	70°	29·68	Ditto
4	6	74°	29·60	Ditto
5	7	78°	29·50	Fine
6	8	68°	29·38	Cloudy
7	9	63°	29·55	Fine
8	10	67°	29·05	Cloudy—heavy rain A. M.
9	11	64°	29·08	Fine
10	12	63·5	29°	Stormy
11	13	64°	29·23	Fine
12	14	68°	29·52	Ditto
13	15	70°	29·44	Cloudy
14	16	63·5	29·35	Fine

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
July 27	61	70	58	30·01	Showery
28	59	63	55	29·99	Hazy
29	58	67	57	30·08	Fair
30	60	67	63	29·96	Rain
31	66	70	64	30·00	Fair
Aug. 1	63	71	63	29·98	Cloudy
2	64	74	62	30·18	Showery
3	63	71	63	·17	Fair
4	64	76	66	·14	Fair
5	66	77	67	·04	Fair
6	68	72	60	29.95	Cloudy
7	62	78	62	30·05	Showery
8	60	62	61	29·72	Rain
9	62	67	55	·58	Cloudy
10	60	65	55	·54	Fair
11	60	68	56	·70	Fair
12	56	67	55	·98	Fair
13	57	70	57	30·04	Fair
14	60	64	55	29·73	Rain
15	57	70	64	30·06	Fair
16	64	72	63	·14	Fair
17	65	73	65	·18	Fair
18	65	68	64	·14	Cloudy
19	58	73	63	·25	Fair
20	59	75	63	·25	Fair
21	60	80	66	·24	Fair
22	64	74	64	·21	Fair
23	63	75	*63	·12	Fair
24	63	80	64	29 99	Fair
25	64	75	65	·98	Fair
26	60	72	60	30;06	Fair

N B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the
13th Aug. 8 o'Clock M. Barom. 30·050 Ther. attached 60° Detached 57
— — 9 — — — — 050 — — — 61 — — 63
— — 1 — — — — ·042 — — — 68 — — 70

XXXVII. *On the new Method proposed by Dr.* YOUNG *for calculating the Atmospherical Refraction.* *By* JAMES IVORY, *M.A. F.R.S.*

IN the last Number of the Quarterly Journal of Science (No. 22), Dr. Young has reprinted what he calls *A Postscript on Atmospherical Refraction*, which was first published in the Philosophical Transactions for 1819. The problem is a very difficult one, and has been treated of by geometers of the first rank; and, in the new point of view in which it is here presented, it is supposed that the principal difficulties have been evaded or overcome. No apology will therefore be necessary, if we endeavour to appreciate the improvement thus achieved in mathematical science, by candidly inquiring how far the pretensions held out are fulfilled.

The leading idea of Dr. Young's method is to develop the density of the air in a series of terms containing the powers of the refraction sought. By this means the problem is brought to the solution of an equation, or to the reversion of a series.

All the methods for computing the refractions that have gained celebrity among astronomers, if we except that of Laplace, are equivalent to the solution of an equation of the second degree. This is true of the rules of Bradley, of Mayer, of Simpson; which are sufficiently accurate for all altitudes within a few degrees of the horizon. It is therefore certain that the two first terms only of Dr. Young's series, namely, those containing the first and second powers of the unknown quantity, will be sufficient for the greater part of a Table of Refractions. The improvement effected by the new method must therefore consist in enabling the calculator to complete the Table, by carrying the refractions quite down to the horizon; for which purpose all the former methods, except that of the French astronomers, are found to be insufficient. The principal point we have to inquire into will therefore relate to the *convergency* of the new series for low altitudes, and more particularly in the extreme case of the horizontal refraction.

Two different ways may be supposed to have occurred to the author for examining the convergency of his series. The most scientific way was to ascertain the rate of the decrease of the terms, by determining the general law of the coefficients. It may be doubted whether this is practicable in the present case, more particularly in the mode of calculation followed by the author. Another way was to take some example about the accuracy of which no doubt existed; and to compare the known result with that obtained from the same data by the new method.

For this purpose the horizontal refraction, on the supposition of a uniform dispersion of heat in the atmosphere, might have been chosen with great propriety, as a case that had already been determined with the greatest accuracy by the calculations of Laplace and Kramp. This very instance is indeed one of Dr. Young's examples; but he employs data different from what the foreign geometers proceed upon; and, on this account, it is difficult to compare the results obtained by the different methods. Besides, the numerical computations in the article in the Journal of Science, are so inaccurate that no conclusion can be drawn from them in which confidence can be placed. Thus, at p. 357, the horizontal refraction on the supposition above alluded to, is brought to an equation of which the solution is said to be $r^2 = \cdot000121$; but the real value which will be found by actual substitution to satisfy the equation, is $r^2 = \cdot0001259$; and hence $r = \cdot011220$, or 38′ 34″, being 44″ greater than the result in the Journal of Science. Again, in a similar calculation, p. 359, the author concludes, $r^2 = \cdot000103$, and $r = 34' \, 53''$; but it should be, $r^2 = \cdot0001066$, and $r = 35' \, 29''$.

The examples given in the Quarterly Journal leave the question of the convergency of the series quite undecided. There can be no doubt that a few of the first terms will in every case enable us to compute the greater part of the quantity sought; but the author has done nothing to determine the precise degree of exactness that will be attained, when only a certain number of the first terms of the series are taken in, and the rest rejected. We shall succeed better in this inquiry if we do not confine ourselves strictly to the mode of calculation imagined by the author. It will conduce greatly to render the theory more accessible, if, by a preliminary investigation, we separate those conditions of the problem that are indispensable from such as are accessary only, and by this means reduce the necessary equations to the least number possible.

Now the fundamental equations of the problem are these two, given § 2, p. 353, viz.

$$u = \frac{S}{1 + pz},$$

$$dr = \frac{du}{v}.$$

In the first of these equations u is the perpendicular falling from the earth's centre upon the direction of a ray of light, or upon the tangent of the trajectory which the ray describes; S, a constant quantity; p, a small fraction expressing the refractive force of the air when the density is unit; and z, the proportional density of the air at the point of the curve from which the tangent is drawn, the density at the surface of the earth being unit.

Let a denote the radius of the earth, and A the apparent altitude of the star: it is obvious that, when the trajectory meets the earth's surface, $u = a \cos A$; wherefore, because $z = 1$, we have, $a \cos A = \frac{S}{1+p}$; whence $S = a \cos A \times (1+p)$. Now, this value of s being substituted, the general equation will become

$$u = a \cos A \times \frac{1+p}{1+pz} = a \cos A \times \frac{1+p}{1+p-p(1-z)}:$$

and from this we readily deduce,

$$\left.\begin{aligned} u &= \frac{a \cos A}{1-\beta\omega}, \\ \beta &= \frac{p}{1+p}, \\ \omega &= 1-z. \end{aligned}\right\} \quad (1)$$

If the light, instead of coming from the star to the spectator, be conceived to proceed in an opposite course from the spectator to the star, u will increase from $a \cos A$ to its ultimate value, while ω increases from zero to unit.

In the second of the fundamental equations, viz. $dr = \frac{du}{v}$; r stands for the angular refraction, and v denotes the part of the tangent between the curve and the perpendicular u. Hence, if x be put for the height above the earth's surface of the point in the curve from which the tangent is drawn, it is obvious that $v = \sqrt{(a+x)^2 - u^2}$; wherefore, $dr = \frac{du}{\sqrt{(a+x)^2 - u^2}}$. If we now substitute the value of u before found, we shall get

$$dr = \frac{\beta \cos A}{(1-\beta\omega)^2} \times \frac{d\omega}{\sqrt{\left(1+\frac{x}{a}\right)^2 - \frac{\cos^2 A}{(1-\beta\omega)^2}}};$$

or, which is the same thing,

$$dr = \frac{\beta \cos A}{1-\beta\omega} \times \frac{d\omega}{\sqrt{\left(1+\frac{x}{a}\right)^2 (1-\beta\omega)^2 - \cos^2 A}}.$$

As the refractive force of the air ceases to be sensible at a height which bears a very small proportion to the earth's semidiameter, $\frac{x}{a}$ will be a very small fraction even at the utmost limits of the atmosphere; wherefore, because β is also very small we may suppose $\left(1+\frac{x}{a}\right)^2(1-\beta\omega)^2 = 1 + 2\frac{x}{a} - 2\beta\omega$; thus,

$$dr = \frac{\beta \cos A}{1-\beta\omega} \times \frac{d\omega}{\sqrt{\sin^2 A + 2\frac{x}{a} - 2\beta\omega}}.$$

Again, the factor $\frac{1}{1-\beta\omega}$ is always between the limits 1 and $\frac{1}{1-\beta}$; wherefore, if we put

$$dr = \beta \cos \Lambda \times \frac{d\omega}{\sqrt{\text{Sin}^2 \Lambda + 2\frac{x}{a} - 2\beta\omega}},$$

we may consider r as the exact refraction; for the true value of the refraction will be between the limits r and $\frac{r}{1-\beta}$, quantities which are so near one another that the difference will in no case amount to half a second.

The differential expression of the refraction now contains only two variable quantities; namely, the height above the earth's surface, and the decrease of the density of the air in ascending to that height. These two quantities are not entirely independent of one another. They are connected by a condition which depends on the pressure, and which we must now investigate. Let y denote the pressure of the atmosphere at the height x, measured by a barometer; and y', the like pressure at the earth's surface. Dr. Young supposes the pressure at the earth's surface to be unit, and uses y to denote the relative pressure at any altitude, equivalent to $\frac{y}{y'}$, when the symbols are taken in the sense here defined. If we suppose x to become $x+dx$, y will become $y-dy$; and the small column of mercury dy will be equivalent in weight to the mass of air $dx \times z$. According to Laplace the elastic force of air at the temperature of melting ice, whatever be the density, is measured by the weight of a homogeneous column equal in altitude to 7974 metres, or 4360·25 fathoms. At any other temperature t reckoned on the centigrade scale; and, allowing that air expands $\frac{1}{250}$ for every centesimal degree of rise of temperature; the length of the homogeneous column that measures the elastic force will be $4360{\cdot}25 \times (1 + {\cdot}004\,t)$ fathoms. Now, t denoting the temperature at the earth's surface, if we put $l = 4360{\cdot}25 \times (1 + {\cdot}004\,t)$, it is obvious that the column of mercury y' will be equal in weight to the column of air l; for each measures the elastic force. Wherefore we shall have this proportion,

$$y' : dy :: l \times 1 : dx \times z;$$

whence, $d.\frac{y}{y'} = -\frac{dx}{l} \times z$. Finally, let $S = \frac{x}{l}$, and $i = \frac{l}{a}$; then $\frac{x}{a} = iS$; and, by substitution, we shall get these two equations which contain all the conditions of the problem, viz.

y

$$\left.\begin{aligned} \frac{y}{y'} &= \int - ds(1-\omega) \\ dr &= \beta \cos A \times \frac{d\omega}{\sqrt{\sin^2 A + 2iS - 2\beta\omega}}. \end{aligned}\right\} \quad (2)$$

In these equations i has the same value with $\frac{1}{m}$ in Dr. Young's Postscript.

Every possible hypothesis relating to the density of the atmosphere; or, which is the same thing, every relation that can subsist between S and ω, must be such, that the integral $\int - ds(1-\omega)$, taken between the limits $\omega = 0$ and $\omega = 1$, must itself extend from 1 to zero. This condition being fulfilled, the second formula will determine the refractions in that constitution of the atmosphere.

According to the method of Dr. Young, we must suppose

$$\omega = Br + Cr^2 + Dr^3 + Er^4 + \&c.$$

Now, if we write Δ for $\sqrt{\sin^2 A + 2\frac{r}{a} - 2\beta\omega}$, we shall get from the last equations, $\frac{d\omega}{dr} = \frac{\Delta}{\beta\cos A}$, by which the coefficient B will be determined, viz. $B = \frac{\sin A}{\beta \cos A}$, because r, s, ω are all evanescent together. Again, take the fluxions of the equation $\frac{d\omega}{dr} = \frac{\Delta}{\beta\cos A}$; thus, $\frac{dd\omega}{dr^2} = \frac{1}{\beta\cos A} \times \left\{ i\frac{dS}{d\omega} - \beta \right\} \times \frac{d\omega}{dr} \times \frac{1}{\Delta}$; but $\frac{d\omega}{dr} \times \frac{1}{\Delta} = \frac{1}{\beta\cos A}$; wherefore,

$$\left.\begin{aligned} \frac{dd\omega}{dr^2} &= \frac{i}{\beta^2\cos^2 A} \times \left\{ \frac{dS}{d\omega} - \lambda \right\}, \\ \lambda &= \frac{\beta}{i}. \end{aligned}\right\} \quad (3)$$

As we must suppose an equation between S and ω from which the value of $\frac{ds}{d\omega}$ will be found, the last formula will determine C, the second coefficient of the series. If we take the fluxions again, we shall get, $\frac{d^3\omega}{dr^3} = \frac{i}{\beta^2\cos^2 A} \times \frac{ddS}{d\omega^2} \times \frac{d\omega}{dr}$; by which the third coefficient D will be determined. And by continuing the like operations all the coefficients of the series may be found. We may also proceed in another way that will bring the determination of the series more immediately under the ordinary rules of analysis. For having an equation between S and ω, we may thence find a value of S, and likewise one of $\frac{dS}{d\omega}$, in terms of

ω; by which means the foregoing equation (3) will be converted into one containing only two variable quantities.

In the case of the horizontal refraction we have $\sin A = 0$, $\cos A = 1$; and, the series for ω containing only the even powers of r, it will be determined by the single equation,

$$\frac{dd\omega}{dr^2} = \frac{\iota}{\beta^2} \cdot \left\{ \frac{dS}{d\omega} - \lambda \right\}.$$

The calculation will be rendered more simple by putting $r = \varrho \times \frac{\beta}{\sqrt{\iota}}$; for then,

$$\left.\begin{aligned} \frac{dd\omega}{d\varrho^2} &= \frac{dS}{d\omega} - \lambda \\ r &= \varrho \times \frac{\beta}{\sqrt{\iota}}. \end{aligned}\right\} \quad (1)$$

and we have now to determine the series,

$$\omega = C\varrho^2 + E\varrho^4 + G\varrho^6 + \&c.$$

In order to bring the question of the convergency to a decision, the best way will be to examine the case of the horizontal refraction in a particular hypothesis; for instance, in that of a uniform temperature prevailing in the atmosphere. In this hypothesis, the densities are proportional to the pressures; that is $\frac{y}{y'} = z = 1 - \omega$. Wherefore the first of the equations (2) will become $(1 - \omega) = \int -ds\,(1 - \omega)$; whence $\frac{dS}{d\omega} = \frac{1}{1-\omega}$. The equation (4) will therefore become,

$$\frac{dd\omega}{d\varrho^2} = \frac{1}{1-\omega} - \lambda = (1 - \lambda) + \omega + \omega^2 + \&c.$$

The coefficients of the series for ω will be determined by substitution, as usual, viz.

$$\begin{aligned} C &= \frac{1-\lambda}{2}, \\ E &= \frac{1-\lambda}{2 \cdot 12}, \\ G &= \frac{1-\lambda}{2 \cdot 12 \cdot 30} + \frac{1}{30} \cdot \left(\frac{1-\lambda}{2}\right)^2, \\ &\&c. \end{aligned}$$

Without carrying the calculation further, we may observe that the series will contain the part,

$$\frac{1}{3 \cdot 4}\left(\frac{1-\lambda}{2}\right)\varrho^4 + \frac{1}{5 \cdot 6}\left(\frac{1-\lambda}{2}\right)^2 \varrho^6 + \frac{1}{7 \cdot 8}\left(\frac{1-\lambda}{2}\right)\varrho^8 + \&c.$$

which, as ϱ^2 is about 2, will converge very slowly. We may therefore conclude with certainty, that the method of calculation proposed by Dr. Young, is deficient in convergency, that is, a few of the first terms of the series, or even a considerable number of them, are not sufficient for computing the refractions with the requisite exactness. It appears therefore that no great improvement of the theory of refraction is to be expected from the new way of considering the subject.

For greater illustration we may apply the foregoing method to the actual calculation of the horizontal refraction, taking the data as they are given in the *Mécanique Céleste;* that is, the mean pressure of the atmosphere being 0·76 metres, and the temperature at the earth's surface, that of melting ice. Then,

$$\beta = \cdot 000293876$$
$$i = \cdot 00125254$$
$$\lambda = \frac{\beta}{i} = \cdot 234625$$
$$C = \cdot 0 \cdot 382625$$
$$E = \cdot 0 \cdot 0318906$$
$$G = \cdot 0 \cdot 0059446$$
&c.

and we have this equation for finding ϱ, viz.

$$1 = \cdot 382625\, \varrho^2 + \cdot 0318906\, \varrho^4 + \cdot 0059446\, \varrho^6;$$

the solution of which is $\varrho^2 = 2 \cdot 10117$; and $\varrho = 1 \cdot 44964$. Hence $r = \frac{\beta}{\sqrt{i}} \times \varrho = \cdot 0120365$, or 41′ 22″; which is 88″ too much, the true quantity being 2394″·6 according to the calculation of Laplace. This great excess arises from the terms of the series that are left out; and, although the error would be lessened, yet, on account of the slow convergency, it would by no means be quite corrected by taking in two or three more terms. There can be no doubt that the calculations, § vii. pp. 357 and 359, likewise bring out results considerably above the truth.

The observations that have been made relate only to Dr. Young's Theory, and do not bear at all upon the Table of Refractions published in the Nautical Almanac 1822. In the explanation annexed to the Table, we are told indeed that it is constructed upon principles explained by Dr. Young in the Philosophical Transactions; but the truth is, that the formula and the Table have very little reference to any theoretical principles, and must both be considered as entirely empirical. The real authority of the Table, or the ground on which its estimation with astronomers must rest, is the manner in which the coefficients have been determined; and upon this point we have very little satisfactory information.

We may suppose that the author of the Table employed two ways for finding the numeral coefficients of his formula. He may have adjusted them to represent some good Table of Refractions, as that of the French astronomers: he may have employed for the same purpose a great number of accurate observations; he may have had recourse to both these methods.

The Table in the Nautical Almanac is easily compared with that in the *Connaissance des Tems*. Both suppose the same mean temperature, 50° of Fahrenheit, and 10° of the centigrade scale. In the English Table the mean pressure of the atmosphere is taken at 30 inches; in the French Table, at 0·76 metres, or 29·92 inches. The numbers in the two tables will therefore be brought to the same circumstances, if those in the French Table be increased by the $\frac{8}{3000}$ or $\frac{1}{375}$ part. When this is done the tables will stand as below:

Altitudes.	*Conn. des Tems.*	Naut. Alm
0°	33′ 51″	33′ 51″
0½	28 37	28 37
1	24 25	24 25
1½	21 5	21 7
2	18 25	18 29
2½	16 16	16 21
3	14 31	14 35
3½	13 3	13 7
4	11 50	11 52
4½	10 49	10 50
5	9 56	9 58

In the remaining parts the two tables agree perfectly with one another. It appears therefore that the French Table is very accurately represented by Dr. Young's formula, the greatest difference being no more than 4″ or 5″ at low altitudes between 1 and 4 degrees. And in like manner, there can be no doubt, a similar formula may be so adjusted as to represent with equal exactness the Table of Bradley, or any other Table of Refractions.

It would be extremely important to be informed, whether a great number of good and original astronomical observations has been employed in constructing the English Table, and what those observations are. If this has actually been the case; if the English Table has a real and solid foundation different from the Table in the *Connaissance des Tems*; it must be allowed that no greater or more honourable testimony can be given in favour of the accuracy of the labours of the French astronomers.

Sept. 4, 1821. J. IVORY.

XXXVIII. *On the aëriform Compounds of Charcoal and Hydrogen; with an Account of some additional Experiments on the Gases from Oil and from Coal.* By Wm. Henry, *M.D. F.R.S.*

[Concluded from p. 93]

Experiments on the Gas from Oil.

In obtaining this gas at different times, I used the same kind of whale oil, which had been heated a little below its boiling point during two hours, in order to deprive it of water. The oil was admitted by drops into an ignited iron tube filled with fragments of broken crucibles, and no difference, that I am aware of, existed in the circumstances under which the decomposition was effected, except that the degree of heat was purposely lowered in the latter processes, till that temperature was attained, which was barely adequate to the production of gas. The oil gas procured from London, I obtained through the kindness of Mr. Richard Phillips. It had been prepared from cod oil, at the manufactory of Messrs. John and Philip Taylor, and having been conveyed to Manchester in bottles accurately stoppered and tied over with a double fold of bladder, it was found not to have acquired any admixture with atmospheric air. The results are contained in the following table, in which the expression *entire gas* is applied to the gas precisely as it came over, except that the carbonic acid had been removed by liquid potash, applied in the smallest quantity and with the least agitation that were adequate to the effect.

Table I. *containing the Results of Experiments on the Gas obtained from Whale Oil.*

	Entire Gas.				Residue left by chlorine.		
			100 vols			100 vols.	
No. of Experiment.	Sp. Gr.	100 vols. lose by chlorine	take oxyg	give carb. ac.	Sp. Gr.	take oxyg	give carb. ac.
	·464	6	116	61	·4107	94	46
	59	19	178	100	·4400	108	58
[illegible]	·7	22·5	220	130	·6160	145	85
4 (London)	·906	38	260	158	·6060	152	91

From the foregoing table it appears, that the gas obtained at different times from oil of the same quality, is far from being of uniform composition, and that great differences, as to its specific gravity and chemical properties, are occasioned by the temperature at which it is produced. So far as my experience goes, no temperature short of ignition is sufficient for the decomposition of oil into permanent combustible gases; but the lower the heat that is employed, provided it be adequate to the effect, the heavier and more combustible is the gas, and the better suited to artificial illumination.

From the experiments which I published in 1805, and which were made on a single specimen of oil gas, I was led to consider it as constituted of one volume of olefiant gas with seven volumes of mixed gases, of which the greatest part was carburetted hydrogen. Mr. Dalton has since favoured me with a specimen of oil gas prepared by himself, which contained in 100 parts, 40 of a gas condensable by chlorine; and it appears from the table that oil gas, manufactured on the large scale, may contain in 100 parts, 38 parts of a gas similarly characterized*. It is not improbable indeed, that, by a temperature carefully regulated, the whole of the aëriform fluids may be obtained from oil, of such quality as to be entirely condensable by chlorine; and from the great superiority of the light which such a gas would afford, and the reduction that might be effected in the capacity of the gasometers, the discovery of a mode of producing it in this state would be an important practical improvement.

The inferences respecting the nature of the gas from oil, I reserve till after the account of the experiments on coal gas, as the same remarks, with some slight modifications, will apply to both cases.

Experiments on the Gas from Coal.

The numerous experiments and observations on the gas from coal, which I have already published, appear to me to preclude the necessity of going much into the subject on this occasion. What I have lately had in view, has been to render the analysis of this gas more complete, by a careful examination of that portion of it which remains after the action of chlorine. The gas, submitted to these recent experiments, was prepared from Wigan cannel, at the manufactory of Messrs. Philips and Lee. It was collected from an opening in a pipe between the retort and the tar-pit, generally about an hour after the commencement of the distillation, except in the instance of the gas No. 4, which was taken five hours, and No. 5, which was taken ten hours, from that period. Before using it, the carbonic acid and sulphuretted hydrogen, which were always present in the early products, were separated by careful ablution with liquid potash. As the gas No. 5 was not at all diminished by chlorine, it was obviously unnecessary to examine it in any but its entire state.

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* Since this paper was written, I have received from Mr. Phillips a second specimen of oil gas prepared by Messrs. Taylor. It contains in every 100 volumes, 42 or 43 parts of gas condensable by chlorine; but in other respects very nearly agrees (making allowance for the greater proportion of that ingredient) with the gas described in the text.

Table II. *Containing the Results of Experiments on the Gas obtained from Coal.*

	Entire Gas.				Gas left by Chlorine.		
		100 volumes				100 volumes	
Experiment.	Sp. Gr.	take oxyg.	give car.ac.	Loss by chlorine.	Sp. Gr.	take oxyg.	give car. ac
1	·650	217	128	13	·575	178	92
2	·620	194	106	12	·527	160	82
3	·630	196	108	12	·535	148	80½
4	·500	166	93	7	·450	140	75
5	345	78	30	0			

Inferences respecting the Composition of that Part of the Gases from Coal and from Oil, which is not condensable by the Action of Chlorine.

The analytical experiments, which I have described on the action of chlorine on artificial mixtures of olefiant with hydrogen and carburetted hydrogen gases, afford no room for doubt that by that agent the quantity of olefiant gas in any mixture of these gases may be accurately determined. We are not, however, acquainted with any chemical agent, either liquid or aeriform, which, from a mixture of hydrogen, carburetted hydrogen, and carbonic oxide, is capable of separating one of those gases, leaving the others in their original state and quantity*. The only method at present known of determining the composition of such a mixture is by firing it with oxygen gas, and from the phænomena and results of the process, deducing the proportion of its ingredients. In drawing conclusions of this kind, it is necessary to have distinctly in view the properties of those gases in their separate state. The following Table contains an abstract of their leading characters, which will be found very useful in such investigations. Though not strictly necessary, I have included olefiant gas, in order to render the Table more complete.

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* I have not found that chlorine can be employed with any success in analysing such mixtures; for when placed in contact with two or more of those gases, and exposed to light, it does not act upon one exclusively, but upon all that compose the mixture.

TABLE III. *exhibiting the Characteristic Properties of different combustible Gases.*

Names of Gases.	Sp Gr. Air 1000.	100 vols. require oxygen.	Total.	Diminished by firing.	Car. Acid produced.
Olefiant gas	·970	300	400	200=$\frac{1}{2}$	200
Carburetted hydrogen	·556	200	300	200=$\frac{2}{3}$	100
Hydrogen gas	·069	50	150	150=$\frac{3}{3}$	0
Carbonic oxide	·972	50	·150	50=$\frac{1}{3}$	100

As an illustration of the method of investigating the proportions of mixtures of the three last gases, we may take the instance of a mixed gas, free from olefiant gas, of specific gravity ·534, of which 100 volumes consume 110 of oxygen, and afford 70 of carbonic acid, the diminution of the whole 210 after firing being 140 volumes. Now it must be obvious from inspection of the Table, that the 70 parts of carbonic acid cannot all have resulted from the combustion of carburetted hydrogen, since, for the saturation of 70 measures of that gas, 140 of oxygen would have been required, whereas only 110 have been expended. We may therefore safely infer the presence of carbonic oxide, a gas which, by combustion, gives its own volume of carbonic acid, with the expenditure of only half its volume of oxygen. The specific gravity of the specimen being lower than that of carburetted hydrogen, indicates also an admixture of simple hydrogen gas; and of this the proportion must necessarily be considerable, to countervail the weight of the heavy carbonic oxide. The following proportions of the three gases will be found to coincide with the properties of the mixture.

	Consume Ox.	Give Carb. Ac.	Dimin. by firing
40 vols. of carb. hydrogen	80	40	80
30 ——— carb. oxide	15	30	15
30 ——— hydrogen gas	15	0	45
100	110	70	140

No reliance, however, can be placed on the accuracy of such estimates, unless the specific gravity of the specimen agrees with that of the hypothetical mixture, as deduced from the proportion of its ingredients. But when this coincidence takes place, we have all the evidence, which the subject at present admits, of the nature of the mixture; and as this agreement between experiment and calculation was found to take place very nearly, in all the instances comprehended in the two following Tables, we may consider the numbers composing them, as expressing, with sufficient exactness, the relative proportion of different gases in the residues of oil and coal gas left by the action of chlorine.

TABLE IV. *showing the Composition of* 100 *Volumes of the Gas remaining after the Action of Chlorine on Oil Gas.*

Exp.	Azote.	Car. Hydr.	Carb.Oxide.	Hydr. Gas.	Total.
1	7	30	15	48	100
2	5	40	15	40	100
3	5	65	20	10	100
4	5	75	15	5	100

TABLE V. *showing the Composition of* 100 *Volumes of the Gas remaining after the Action of Chlorine on Coal Gas.*

Exp.	Azote.	Car. Hydr.	Carb.Oxide.	Hydr. Gas.	Total.
1	1·5	94·5	4	0	100
2	6	82	2	10	100
3	2	66	14	18	100
4	5	60	12	23	100
5	10	20	10	60	100

It appears from the two foregoing Tables, that the portion of oil gas and coal gas, which is not condensable by chlorine, is in every case a mixed gas, consisting in most instances of carburetted hydrogen, carbonic oxide, and hydrogen, with a little azote, part of which may be traced to the impurity of the chlorine. In the best specimens of oil gas, the carbonic oxide is in greater proportion than in the best kinds of gas from coal, and the carburetted hydrogen is most abundant in the latter gas. This, however, is more than compensated, so far as their illuminating power is concerned, by the greater richness of the aëriform products of oil in that denser species of gas which is separable by chlorine. The proportion of hydrogen, both in oil gas and coal gas, appears to increase as they are formed at a higher temperature, and is always greatest in the latter portions of the gas from coal. But no instance has ever occurred to me of a gas obtained from oil or from coal, which, after the action of chlorine upon it, with the exclusion of light, presented a residuum at all approaching to simple hydrogen gas; nor do I believe that such a gas can be generated under any circumstances of temperature, by which the decomposition of coal or of oil is capable of being effected.

Inferences respecting the Composition of that Part of the Gas from Coal and Oil, which is condensed by contact with Chlorine.

When a given volume of a mixture of olefiant and carburetted hydrogen gases is fired with oxygen, and an equal volume of the same mixture is first deprived of olefiant gas by the action of chlorine, and then fired with oxygen, it must necessarily happen that the excess of oxygen spent in the first combustion, above that consumed in the second, will be three times the volume of the olefiant gas, and that the excess of carbonic acid formed in the first experiment above that generated in the second, will be double the volume of the olefiant gas. A remarkable anomaly, however, was during the last summer observed by Mr. Dalton in the results of the combustion of a quantity of gas, which he had himself prepared from oil. One volume was found to consume three volumes of oxygen, and to yield little short of two volumes of carbonic acid, in those respects agreeing nearly with olefiant gas; but when mingled with more than the requisite proportion of chlorine, it was not, as olefiant gas would have been, entirely condensed, but suffered a diminution of only four-tenths of its bulk, the remaining six-tenths, after being freed from the redundant chlorine, agreeing in its properties with carburetted hydrogen. For example, 10 volumes of this gas (containing four of gas condensable by chlorine and six of carburetted hydrogen) consumed 30 volumes of oxygen, and gave 18 of carbonic acid. But of the oxygen, 12 volumes are due to the six of carburetted hydrogen, leaving 18 volumes for the combustion of the four volumes of gas condensable by chlorine, which is in the proportion of 4½ to 1. Of the 18 volumes of carbonic acid, also, six may be traced to the combustion of the carburetted hydrogen, leaving 12 volumes as the product of four of the condensable gas, or in the proportion of 3 to 1. The portion of gas condensed by the action of chlorine presents, therefore, decided differences from olefiant gas, in requiring not three only, but 4½ volumes of oxygen for combustion, and in affording 3 instead of 2 volumes of carbonic acid. Nearly the same relation of the oxygen consumed, and carbonic acid produced, to that part of the gases from coal and oil which is condensable by chlorine, existed also not only in other experiments of Mr. Dalton, but in all those which I have myself made. The proportions I have found to vary in different cases from 4½ to 5 volumes of oxygen, and from 2½ to 3 volumes of carbonic acid for each volume of the condensable gas.

On comparing also the specific gravity of the gases from coal and oil, as ascertained by experiment, with that which ought to result from mixtures of the residue left by chlorine, with such a proportion of olefiant gas as is deducible from analysis, I have invariably found, that the real specific gravity has considerably exceeded the estimated. For instance, the London oil gas was composed of 38 volumes of a gas condensable by chlorine, and

62 volumes of mixed gases not characterized by that property; and having the specific gravity ·606. But 62 volumes of gas of specific gravity ·606, mixed with 38 volumes of olefiant gas of specific gravity ·970, should give a mixture of the specific gravity ·754, instead of ·906, which was the actual specific gravity of the entire oil gas. It will be found on calculation that the 38 volumes of gas, in order to make up the real specific gravity of the oil gas, must have had the specific gravity of 1·4 very nearly. This is the highest number that is deducible from my experiments for the specific gravity of that portion of oil gas, or coal gas, which is condensed by the action of chlorine. In other instances, it varied from that number down to 1·2, but in every case its weight surpassed that of common air.

It is evident from these facts, that the aëriform ingredient of oil gas and coal gas, which is reducible to a liquid form by chlorine, is not identical with the olefiant gas obtained by the action of sulphuric acid on alcohol, but considerably exceeds that gas in specific gravity and combustibility. Two views may be taken of its nature; for it may either be a gas *sui generis*, hitherto unknown, and constituted of hydrogen and charcoal in different proportions from those composing any known compound of those elements;—or it may be merely the vapour of a highly volatile oil, mingled in various proportions with olefiant gas, carburetted hydrogen, and the other combustible gases. Of these two opinions, Mr. Dalton is inclined to the first, considering it as supported by the fact that oil gas, or coal gas, may be passed through water without being deprived of the ingredient in question; and that this anomalous elastic fluid is absorbed by agitation with water, and again expelled by heat or other gases, unchanged as to its chemical properties, as we have both satisfied ourselves by repeated experiments. On the other hand, I have found that hydrogen gas, by remaining several days in narrow tubes in contact with fluid naphtha, acquires the property of being affected by chlorine precisely as if it were mixed with a small proportion of olefiant gas; and I am informed by Dr. Hope, that oil gas, when forcibly compressed in Gordon's portable gas lamp, deposits a portion of a highly volatile essential oil. The smell also of the liquid which is condensed on the inner surface of a glass receiver in which oil gas or coal gas has been mixed with chlorine, denotes the presence of chloric ether, evidently however mingled with the odour of some other fluid, which seems to me to bear most resemblance to that of spirit of turpentine. This part of the subject is well worthy of further investigation; but having devoted to the inquiry all the leisure which I am now able to command, I must remain satisfied at present with such conclusions as are safely deducible from the foregoing investigation. These may be briefly recapitulated as follows:

1. That carburetted hydrogen gas must still be considered as a distinct species, requiring for the perfect combustion of each volume two volumes of oxygen, and affording one volume of carbonic acid; and that if olefiant gas be considered as constituted of one atom of charcoal united with one atom of hydrogen, carburetted hydrogen must consist of one atom of charcoal in combination with two atoms of hydrogen.

2. That there is a marked distinction between the action of chlorine on olefiant gas, (which, in certain proportions, is entirely independent of the presence of light, and is attended with the speedy condensation of the two gases into chloric ether,) and its relation to hydrogen, carburetted hydrogen, and carbonic oxide gases, on all which it is inefficient, provided light be perfectly excluded from the mixture.

3. That since chlorine, under these circumstances, condenses olefiant gas without acting on the other three gases, it may be employed in the correct separation of the former from one or more of the three latter.

4. That the gases evolved by heat from coal and from oil, though extremely uncertain as to the proportions of their ingredients, consist essentially of carburetted hydrogen, with variable proportions of hydrogen and carbonic oxide; and that they owe, moreover, much of their illuminating power to an elastic fluid, which resembles olefiant gas in the property of being speedily condensed by chlorine.

5. That the portion of oil gas and coal gas, which chlorine thus converts into a liquid form, does not precisely agree with olefiant gas in its other properties; but requires, for the combustion of each volume, nearly two volumes of oxygen more than are sufficient for saturating one volume of olefiant gas, and affords one additional volume of carbonic acid. It is probably, therefore, either a mixture of olefiant gas with a heavier and more combustible gas or vapour, or a new gas *sui generis*, consisting of hydrogen and charcoal in proportions that remain to be determined.

Manchester, Jan. 1821.

XXXIX. *On Mr.* CARNOT'S *new System of Defense of Places by what he calls Vertical Firing.*

SOME years past, Mr. Carnot, a celebrated French mathematician and military engineer, published a work on a new mode of defence of forts against an enemy besieging the place when he has got possession of the ditch, where the guns of the fort cannot

not be pointed so as to touch him, on account of his closeness to the rampart. In order therefore to annoy the enemy in that position, Mr. Carnot invented a new system of attacking him there by what he calls Vertical Firing, which has obtained great applause by engineers on the continent of Europe. This method is described as a mode of discharging a multitude of small balls from cannon pointed upwards, nearly in the vertical direction, so that the balls, after ascending to their utmost height, may fall down again like a shower of hail, on the heads and shoulders of the men in the ditch. It seems that those engineers imagined that these showers of balls, in their descent, would fall to the ground with a velocity or force equal to that with which they were discharged from the cannon, and that as the latter is capable of destroying men, the former must likewise have the same effect.

But this it appears is a very vain and fallacious opinion, as, owing to the enormous resistance of the air to bodies moving with great velocity, being indeed even more than in proportion to the square of the velocity, the utmost velocity of the descending balls is comparatively very small and harmless. This circumstance is fully demonstrated in Dr. Hutton's artillery experiments, as well as many others, in the 2d and 3d volumes of his Tracts, where rules are delivered to assign the utmost velocity that can be acquired by bodies, of any size and weight, falling through the air from any height whatever, and therefore called *the Terminal Velocity*. In particular, by consulting the table of such velocities, in page 247 of the 3d volume above mentioned, it will appear that such balls cannot acquire a velocity, by descending, of so much as 200 feet per second of time, even if they were discharged upwards with ten times as much.

This grand, this *fatal* mistake of the continental engineers, having been observed by Sir Howard Douglas, Bart. one of the many able engineers educated by Dr. Hutton, at the Royal Military Academy at Woolwich, lately Inspector General of the Royal Military College, and now Inspector of the Hon. East India Company's Military Institution at Addiscombe near Croydon, he lately published a work, entitled "Observations on the Motives, Errors, and Tendency, of Mr. Carnot's Principles of Defence," &c. meant to expose and correct the error of that system, and prevent the fatal consequences that were likely to attend it; and which it seems is now in a fair way of being completely effected, as appears by the following letter just received by Dr. Hutton, from that gentleman, who is now with his family at Caen in Normandy:

" *To Dr.* HUTTON, *Bedford Row, London.*

" Caen, Aug. 13, 1821.

" MY DEAR DOCTOR,—I have just received a French *Mémoire*, sent to me by the author, which you will have great pleasure in reading.

" I dare say General Rowley will soon have a copy of it, and will lend it to you.

" It is entitled 'Mémoire sur l'Effet des Feus verticaux proposés par M. Carnot: par M. Augoyat,' of the French Engineers, Professor of Fortification in the chief Military College of France. The author, in a close investigation of Mr. Carnot's system of defence, with reference mainly to my work, and to your Tracts, has admitted all my reasoning, and adopted all my conclusions. At the end of the *Mémoire* are two articles, which make such honourable reference to your works, as cannot fail to give you much gratification in perusing.

" The attention of the French engineers to the controversy respecting Carnot's system of defence, has been excited by two causes; viz. to determine how far his ideas should be acted upon in their own fortifications, and to teach them how to estimate the powers of resistance of those works which the Prussians are constructing at Coblentz and Cologne, on this most defective plan. The former is set at rest; for the *Mémoire* may be considered as passing sentence upon Mr. Carnot's System, and that by a body of men certainly not prejudiced against him, nor in favour of a British military author. And with respect to the other bearing of the question, the French engineers are satisfied that the Prussians are acting upon an insecure and condemned system.

" This *Mémoire* gives some strength to my cause, and I will endeavour to push it further to deter the Prussians from proceeding with this defective scheme.

" I hope this will find you in perfect health. I shall be in England in about two months.

" Believe me, dear Doctor,

" Yours very sincerely and truly,

" HOWARD DOUGLAS."

XL. *On a new graphical Method of reducing the Lunar stances. By Mr.* HENRY MEIKLE.

To Dr. Tilloch.

SIR,—ON attentively considering the different methods hitherto used for reducing the lunar distances by mechanical or graphical means, I have often thought it would be much more convenient if a general method could be found in which all the lines were ready drawn without requiring scales, compasses, or even different plates; for although in Margett's tables and some others, no new lines are wanted, still each plate only serving for a small part of the various cases that may occur, many plates are necessary to comprehend the whole; and in the different editions of La Caille's method, some moveable parts are required, as well as several accurate measurements with compasses, &c., operations not only troublesome, but in unskilful hands they are apt to produce errors in abundance.

What I was in quest of, therefore, was the construction of a general plate by which all the cases might be solved without the aid of any thing else, except a common ruler to lay across the plate; and my researches on the subject have upon the whole been fully as successful as I had at first expected; but it probably admits of still further improvement.

The outline of all the other methods that I have seen for solving the problem* by projection, especially so far as relates to parallax, is the same; viz. the orthographical projection of the spherical triangle formed by the distance and the complements of the altitudes, upon the plane of the circle of which the distance forms a part. The one I am about to describe, however, is entirely different. In this there is given a separate contrivance for the effect of refraction and another for that of parallax: but both can still be conveniently put in the same plate without enlarging its size. I once intended to have combined the two in one correction [k], which was not impossible; but afterwards thought it better to abandon that idea, because it led to some inaccuracy, arising from the necessity of varying the effect of refraction in the same ratio as the horizontal parallax; and I wished to give a method founded on principles admitting of some degree of exactness, should I afterwards have occasion to construct it on a large scale.

I shall now proceed to explain the principle on which the correction for refraction is founded:—Assuming the refraction as the cotangent of the altitude, it may easily be shown that, on a given distance, the effect of refraction is the same for any two altitudes whose sines have the same ratio. If neither altitude is under 10° this assumption cannot materially err; but if we augment each apparent altitude by nearly three times the corresponding refraction, it will be brought sufficiently near the truth for our present purpose almost at the horizon.

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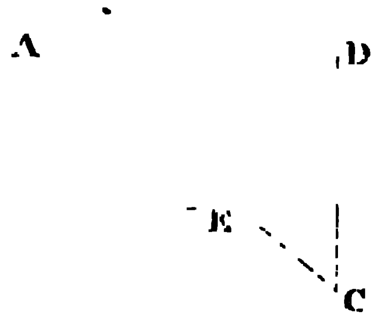
* Of the methods in which all the lines are to be drawn for each case, Dr. Kelly's perhaps comes as near the truth as any similar one could requiring so little labour. In it the refraction is combined with parallax An improved method of the same kind has lately been given in Professor Brande's Journal.



Let A B and C D be two parallel lines of sines whose zeros are at A and C. Join AC, as also F and H the two altitudes increased as above, to cut AC in E. Then it is plain that the line joining any other altitudes whose sines are in the ratio of A F to C H must also pass through E. If, therefore, E I be the effect of refraction on a given distance in the first case, it will be so whenever the sines have the same ratio: reckoning always the greater altitude on A B.

The construction of this part is as follows:—Having drawn and divided the lines of sines, take any distance which we shall imagine to be an arc in a vertical circle, in order that the effect of refraction may be had at once from a table of refractions; since in that case it is the sum or difference of the refractions corresponding to the altitudes; and having laid a ruler to join these altitudes, let this effect be set off in a straight line as from E to I.

Suppose again that we shift round the same arc of distance, still keeping it in the vertical circle, till the sines of the altitudes have a different ratio; we may then find the effect of refraction as before; and proceeding in this way for all ratios of altitude with the several distances, the linear table may be completed to a considerable degree of accuracy without requiring any other calculation. The effect of refraction might be expressed in various ways; but perhaps one of the most convenient is to do it by parallel straight lines such as E I reaching from AC to a curve which belongs to the corresponding distance. The arguments of this table are simply the apparent altitudes and apparent distance; because the numbers for the apparent altitudes are to be placed opposite the sines of the apparent altitudes increased by thrice the refraction. The correction for refraction is thus obtained in any given case, by merely applying a ruler to the altitudes; this will cut AC in a point between which and the curve corresponding to the distance, the required correction is contained on a straight line such as E I. It is always additive.

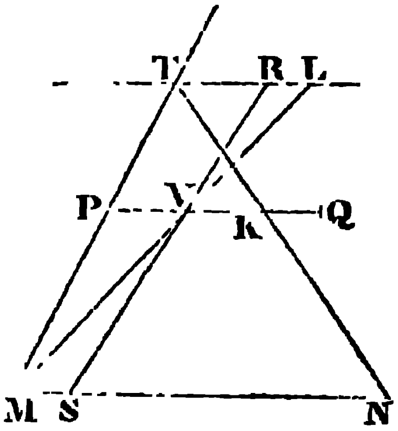
I shall next explain the principles of the part that relates to parallax: Let M N and P Q be two parallel straight lines, of which M N is the greater; join M P. Take $\frac{PK}{PQ} = \cos. d$, the distance being denoted by d; join N K, producing it to meet the extension of M P in T. Through T draw T L parallel to P Q;

make $\frac{MS}{MN} = \sin m$, and $\frac{PV}{PQ} = \sin s$; the moon's altitude being $= m$, and that of the star $= s$. Join MV, SV, and produce them to meet TL in L and R. Then on account of the parallels $LR : MS :: LV : MV$, and $PV : LT :: MV : LM$; hence $LR \times PV : LT \times MS :: LV : LM :: PK : MN$, and $LR : LT :: MS \times KP : PV \times MN :: \sin m . \cos d : \sin s$.

Consequently with *a given distance*, RT varies as $\sin m . \cot d - \sin s . \operatorname{cosec} d$. That is, as the cosine of the angle at the moon multified by the cosine of her altitude: a well known expression for the principal effect of her parallax on the distance, supposing the horizontal parallax to be unity.

This correction is subtractive when R lies to the right of MT, otherwise it is additive. When $m = o$, and the distance is in a vertical circle, RT becomes a maximum for the subtractive correction, and represents the horizontal parallax; also if $s = o$, and the objects are in the same vertical quadrant, RT will show the greatest additive correction for parallax that the corresponding distance admits of.

Corresponding to the sign of cos *d*, it is evident that LT will lie above or below PQ, as also K will be on the right or left of P, according as the distance is less or greater than 90°. However, since the *parallels of distance* or successive positions of LT, become somewhat crowded as the distance approaches 120°, it might perhaps be better after it exceeds 90°, to transpose the altitudes, reckoning the moon's on PQ, and that of the star on MN. In this case the parallel of distance 90° would coincide with MN, and the rest be continued upward from it till they reached the place of 120°. By this means the scale would be considerably enlarged, and the confusion of having PQ with its divisions of sines crowded in the midst of the other parallels would also be avoided.



There are different positions in which two lines of sines might be permanently placed to give a construction for solving the problem. When these lines are parallel but lie in contrary directions, the parallels of distances less than 90° fall between them, but are excessively crowded and contracted as the distance approaches 20°,—the least in common use. For distances greater than 90°, the parallels lie without these reversed lines of altitudes. Thus if MN were produced beyond M, and another equal line of sines laid off from M in the opposite direction, by using this for the moon's altitude, we could read off the effect of parallax for a distance of 120° on the same parallel that is used for 60°; and in a similar way for those between 90° and 120°. But it is obvious that M N thus produced would be inconveniently long. It is true, however, that M N being merely reversed, or rather another equal line of sines laid close to it but beginning from N, by using this for the moon's altitude when the distance exceeds 90°, we might still have the correction on the parallel of 60°, &c.; but then neither the divisions nor numbers attached to them would suit well. The line L T, we may also observe, might have one permanent position, while the lines of altitude changed their places or even magnitudes for each degree of distance; but this would likewise be attended with several inconveniences.

Preferring then the former arrangement, and that part of each parallel of distance lying to the right of M T being divided into 60 equal parts with the same divisions continued as far as necessary to the left, we shall obtain the effect of parallax in any given case, merely by applying a ruler to the two altitudes, and then the segment of the parallel of the distance intercepted between the ruler and M T will be the required correction in minutes, supposing a horizontal parallax of 60°. This number again being multiplied by the given horizontal parallax, and divided by 60, gives a quotient in minutes and a remainder in seconds corresponding to the given horizontal parallax.

In all this we have assumed the effect of parallax as strictly proportional to the cosine of the angle at the moon: this however is not in general quite correct. The error is usually denominated the final correction, and is contained in the 13th of the Requisite Tables. It is nearly proportional to the cotangent of the distance multiplied by the difference of the squares of the parallaxes in altitude and distance.

In the expression, sin m cot d, if m be altered by a quantity $\acute{m}$ proportional to the parallax in altitude, the change in the correction is as $\acute{m}^2$ cot d, which is proportional to one term of the final correction. The other term may be allowed for, by everywhere shifting the divisions of R T by a small quantity proportional to its square. If however this method of making out the final correction be used, it is evident we cannot transpose the altitudes in the manner already proposed, because their lines of sines would not then be quite similarly divided.

But the final correction may be effected with almost sufficient accuracy for a thing of this kind, by merely curving a little the parallels of distance and making their divisions somewhat unequal.

equal. Indeed for distances greater than 60°, such a correction is scarcely worth noticing, unless the scale of the projection be very great.

If it were wished to construct a plate of this kind on a large scale, it might tend to ensure still greater accuracy in the part that relates to parallax, if in place of the sines of the apparent altitudes, we use the sines of the altitudes corrected for refraction; attaching however still to these lines as their arguments the apparent altitudes only: so that no additional trouble will occur in the use of the plate, except that the correction for refraction should be applied to the distance before finding that of parallax. But after all, these niceties would seldom have any sensible effect. The part for the correction of refraction might be brought to give the effect of the mean refraction to almost any degree of exactness, by slightly curving the three principal lines.

The foregoing speculation, it is presumed, will be found to contain principles for constructing a *single* plate whereby all the cases may be solved with scarcely less facility or accuracy than the *seventy* plates of Margetts. A plate of the kind I intend shortly to publish; but previous to doing so, I shall endeavour to try of what further improvement it is susceptible.

I am, sir,
Your most obedient servant,
July 16, 1821. HENRY MEIKLE.

XLI. *On* SHOOTING STARS, *and Meteors which throw down* METEOROLITES, *as distinguished from fiery Appearances low in the Atmosphere, which have been supposed to proceed from terrestrial Exhalutions, and to prognosticate Wind and Rain, &c.; with Directions for observing Shooting Stars. By Mr.* JOHN FAREY *Sen.*

To Dr. Tilloch.

SIR, — I BEG to thank Dr. William Burney for his prompt and very obliging attention to my request, regarding *Barometric Observations* on the second Monday of each Month; and hope that he and others of your ingenious Correspondents will persevere therein, and let slip no favourable opportunities of procuring such observations to be simultaneously made, at as many points as possible on the open Coasts of the Ocean, at ascertained heights above *high and low water* marks on the days of observation.

My best acknowledgements are also due to Dr. Burney, for his obliging notices of my Queries (in p. 346 of your last Volume) in pages 22 and 127 of your late Numbers; from a careful perusal of which I perceive, with regret, that when drawing up those Queries, and in some other papers which I have written on the subject, I have not been sufficiently on my guard, to expressly exclude from my description, of those Shooting-stars and Meteors seen high in the Atmosphere (to which I have been desirous of ascribing a satellitic origin) those lower, fiery appearances, locally and occasionally seen in the Air, to which some observers' attention is directed, and which they think to arise from gaseous exhalations from the Earth; amongst which class of phænomena, the *ignis fatuus* is an undoubted instance. Others have called in the aid of Electricity, to account for other luminous atmospheric appearances, citing, and perhaps truly, the *aurora borealis*, as one of the same class of phænomena with *Lightning*, especially that which on sultry evenings, sometimes appears almost incessant and universal, yet unaccompanied by thunder.

I seem to have been mistakenly supposed by Dr. B. to maintain, that not any Meteors are visible in full moon-light; whereas I believe many fiery appearances in the Atmosphere, besides Lightning, including some Meteors which have been in the act of exploding and throwing down Meteorolites, to have been seen in full day-light: and yet maintain this to be, noways inconsistent with the opinion, that real *Shooting-stars* of the smaller classes, are rendered invisible, by very small degrees of extraneous light in their vicinity, whether such be from bright planets, or from the Moon almost ever so near her change; such obscuration of the faint light of small Shooting-stars, extending considerably further round the one-day's Moon, much further yet around the two-days Moon, and in a short time afterwards in the lunation, whenever the Moon is above the horizon, the smaller Shooting-stars may in vain be watched for, in any part of atmospheric space.

There are resident in the Country, many curious Gentlemen, who have it in their power to procure the assistance of two and occasionally of a third steady person (such for instance as their domestics or clerks) in the first two or three hours after dark, in the Evenings which may prove free from general clouds; who are provided with a good pendulum Clock, which by a transit or other Instrument they have the means of regulating, and also with a Watch carrying a second's Hand: and who either possess or could make a large Planisphere, in portions overlapping each other, and depicting the smaller Stars, of such a zone of the heavens, as within the course of a year passed in convenient view, from some window of their House, adapted for the scene of Stellar observations.

To the zeal of such Gentlemen for the extension of knowledge, I beg to address myself, and to request the favour of their concurrence, in steadily making, and recording, through a sufficient period of time, *Observations on the Shooting Stars and Meteors,* which may present themselves to the Eye: such not being made to wander about the field of view, in search of moving objects, but kept steadily fixed on some one constellation, or on a remarkable Star, not too bright, which through the successive Evenings of three or four weeks, may be fixed on, as the centre of the observations to be made.

One of the Assistants should be seated before the Watch, carefully following the second's Hand with his Eye, and inwardly counting the seconds clapsed, since each successive minute: another Assistant should be seated with the Planisphere before him, having his Eyes occasionally directed to the central Star agreed on as above mentioned, with Paper also before him, and Pen in hand, ready to write down instantly, any observations which may be dictated to him, and to trace with the pencil on the Planisphere.

Things being thus arranged, preparation may be made for commencing the Observations, by noting down the date and the time; with the addition of all observable circumstances, regarding the clearness of the air or otherwise, the situations and nature of any clouds in the field of view, the age and situation of the Moon, and whether its light or that of any of the brighter planets, seemed likely to affect the observations.

The observations may then commence, by the Observer standing or sitting at ease, and fixing his eyes on the Star agreed on, steadily, but not with any straining or effort which would fatigue the Eye:—at the very instant of seeing any Star move, he should cry out "now," or any other short word that may be agreed on, as a direction to the Assistant at the Watch, to repeat aloud *the second* he had last counted, with the addition of one, two or three, quarter seconds clapsed since; these seconds and fractions the other Assistant should instantly write down, in the proper marginal column of his Paper, and then place before them, the hour and minute, which next the Watch Assistant should read aloud.

In the mean time, the Observer having as quickly as possible transferred the axis of his Eyes to the moving Star, he should make an effort to follow exactly its track with his Eyes, at the same time being very attentive to observe and remember, against or near to what fixed Star, he first saw the movement, what particular Stars it may pass in its course, and near to which of them it vanishes: as soon as this has happened, he should begin to mention these Stars' names (or characters or numbers, as they are distinguished in the Planisphere), which the Assistant should write down, after tracing the apparent course on the Planisphere, by means of a soft Pencil.

The Observer should then dictate, for the Assistant to write down, the rough *apparent direction*, as for instance, "from left to right, level," "from right to left, inclining a little upwards," "from above downwards, direct," "from below upwards, inclining much to the right," &c.: next he should mention, whether the apparent course was straight or otherwise, whether long or short; whether the object was minute, small or comparatively large; whether faint, bright or brilliant; all which, and any other observable circumstances, being noted down, this observation may be considered as finished.

It will be desirable, that the Observer while dictating these circumstances to the writing Assistant, should again have his Eye fixed on the central Star, in readiness to notice, the *time* and rough *direction*, at the least, of any other moving Body, which he may not be able to notice further, without endangering confusion, or the loss of observations already made, but not recorded.

A third Assistant at hand, will be very useful, to relieve instantly the Observer, when tired, or having occasion to consult the Planisphere, or revise the observations already written down, &c.; or in case of two Assistants only being present, it may be necessary that one of them should occasionally relieve the Observer.

When the one, two or three Hours allotted to these observations, shall have concluded, or the coming on of Clouds may sooner terminate the Evening's observations, the hour and minute of such termination should be noted down: and after the pencillings on the Planisphere shall have been compared, with the written descriptions of the apparent courses, the same may be rubbed out, ready for the next night's operations.

I shall not at this time trespass further on your pages, by mentioning such preliminary arrangements and precautions as will be necessary, when simultaneous observations are intended, by two Observers, situate at several Miles distance from each other, because most of these are sufficiently alluded to already, in my Queries referred to.

I am, sir,
Your obedient servant,

37, Howland-street, Fitzroy-square
Sept. 9, 1821

JOHN FAREY *Sen.*

XLII. *An*

XLII. *An Address to a Phrenologist.* *By* A CORRESPONDENT.

"If not to some peculiar end assign'd,
Study 's the specious *trifling* of the mind."—YOUNG.

THE craniological, now phrenological, opinions of Drs. Gall and Spurzheim again draw the attention of some individuals in this country, and the following remarks on these opinions may perhaps not be altogether unworthy of their attention.

Address to a Phrenologist.

Take it for granted that your opinion regarding the organization of the brain be correct, and that in the head of each individual there are certain indications of certain qualities, affections, or powers of that individual's mind; does it not follow that these indications and the qualities they indicate, are either necessarily existent in the individual from his first formation? or the effect of peculiar associations of ideas at a very early period in life?

1. If the former be your opinion, do you mean to say that in the formation of man certain qualities are united, and that his head, like a mineral crystal, is of a determinate form according to the proportions of its constituent elements? If so, does not your doctrine lead to fatalism? and its promulgation will be dangerous to society, because, to some minds it may be made plausible by a continual reference to an apparent agreement with physical observations; and particularly in minds that have not a ready antidote of truths drawn from more legitimate sources.

2. If you say that these indications are the effect of peculiar associations, you have only found a very imperfect means of knowing that which is apparent, as far as it is useful, to common observation. Perhaps your system may strengthen the organ of suspicion in those who have it, and sometimes create it where it was not: how desirable this may be, you are left to judge.

It is true that certain muscles in the arm of a smith gain more than ordinary strength by constant action under a strain in one direction; that those of a cobler acquire a peculiar set from a similar cause; and it may be, that the exertion of certain powers of the mind may enlarge certain parts of the brain. But would you study the laws which move the ocean in the impressions it makes on the sea-shore? Rather let the mind itself be the direct object of your inquiries; let the laws of its action be your study; the force and peculiar direction of that action is always sufficiently demonstrated in the individual's intercourse with the world.

Viewing your system through the medium of utility, I think it will appear to be decidedly hostile to the best interests of society; or, at the most, only an object of disagreeable curiosity. Your presence may excite alarm in a weak mind; such a mind may consult you as our forefathers did a Lilly, or a Dee; but superior minds will regard you with a mixture of compassion and contempt[*]. Take counsel, and follow another course; the field of useful knowledge is wide, it is of a rich soil, and yet affords but a small produce from the want of labourers that are willing to abandon altogether toy-making, and the construction of ingenious riddles.

Essex, Sept. 14, 1821. D——T.

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[*] If you be eminent in any branch of human knowledge, beware how you engage with phrenology, for the foibles and false steps of superior minds are sometimes recollected when their real excellencies are nearly forgotten or kept out of sight; you will find an instance of this in a description of Merchiston Tower, in a late Number of the "Provincial Antiquities of Scotland." In that description, those fathers of science, Newton and Napier, have their foibles exposed with too free a hand, when it is considered that the authority of this popular writer will be gladly seized by men who would willingly sink the greatest talents to the level of their own. The writer seems also to be in error when he makes Napier's bones a name of logarithms.

XLIII. *Tables of the Longitude and Altitude of the Nonagesimal Degree of the Ecliptic. By Mr.* JAMES URTING.

To Dr. Tilloch.

SIR, — I HAVE sent you for insertion in the Philosophical Magazine and Journal, a Table of the Longitude and Altitude of the Nonagesimal Degree of the Ecliptic. As the finding of the longitude and altitude of the Nonagesima is an extremely tedious operation, I presume the Table I have calculated will be found valuable, more particularly as the Tables inserted in the first volume of Dr. Maskelyne's Astronomical Observations made at the Royal Observatory at Greenwich, and in Vince's Astronomy, give the longitude and altitude only to within 10 seconds (being calculated before Taylor's Tables of Logarithms were published), and moreover contain no correction for the Variation of the Obliquity of the Ecliptic, and but a very brief Table of the correction necessary to be made for a change of latitude.

Longitude and Altitude of the Nonagesimal Degree of the Ecliptic, for the Latitude of the Royal Observatory at Greenwich, 51° 28′ 40″ North, or 51° 17′ 48″ reduced to the Earth's centre, the Ellipticity of the terrestrial Spheroid being 1-309th part of the Equatorial Radii. With the Variations of Longitude and Altitude for 100 Minutes of Latitude North of Greenwich; and for 100 Seconds Diminution of the Obliquity of the Ecliptic. (Obliq. Ecliptic 23° 28′) Calculated from Taylor's Tables of Logarithms.

ARGUMENT. *Right Ascension of the Medium Cœli.*

R.A. of M. Cœli.	Longitude of Nonagesima.				Altitude of Nonagesima.			
	Longitude.	Diff.	V. for 100′ V. lat. N.	V. 100″ V. of Ob.	Altitude.	Diff.	V. for 100′ Var. of lat.	Var. 100″ V. of Ob.
°	s ° ′ ″	° ′ ″	° ′ ″ +	′ ″ −	° ′ ″	′ ″	° ′ ″ −	′ ″ −
0	0 26 25 38	0 44 3	1 23 45	1 32	44 17 11	21 20	1 21 39	0 45
1	0 27 9 41	0 43 56	1 22 40	1 30	44 38 31	21 11	1 21 55	0 46
2	0 27 53 37	0 43 47	1 21 35	1 29	44 59 45	21 3	1 22 11	0 47
3	0 28 37 24	0 43 41	1 20 31	1 27	45 20 48	20 54	1 22 28	0 48
4	0 29 21 5	0 43 33	1 19 27	1 25	45 41 42	20 45	1 22 44	0 49
5	1 0 4 38	0 43 26	1 18 24	1 24	46 2 27	20 35	1 23 0	0 50
6	1 0 48 4	0 43 20	1 17 21	1 22	46 23 2	20 27	1 23 17	0 51
7	1 1 31 24	0 43 13	1 16 19	1 20	46 43 29	20 18	1 23 34	0 52
8	1 2 14 37	0 43 8	1 15 17	1 19	47 3 47	20 7	1 23 51	0 53
9	1 2 57 45	0 43 2	1 14 17	1 17	47 23 54	19 58	1 24 8	0 54
10	1 3 40 47	0 42 57	1 13 16	1 16	47 43 52	19 48	1 24 25	0 56
11	1 4 23 44	0 42 52	1 12 16	1 14	48 3 40	19 38	1 24 42	0 57
12	1 5 6 36	0 42 48	1 11 16	1 13	48 23 18	19 28	1 25 0	0 58
13	1 5 49 24	0 42 43	1 10 17	1 11	48 42 46	19 16	1 25 17	0 59
14	1 6 32 7	0 42 40	1 9 17	1 10	49 2 2	19 7	1 25 34	1 0
15	1 7 14 47	0 42 36	1 8 18	1 8	49 21 9	18 56	1 25 51	1 1
16	1 7 57 23	0 42 31	1 7 19	1 7	49 40 5	18 45	1 26 9	1 2
17	1 8 39 51	0 42 29	1 6 21	1 6	49 58 50	18 33	1 26 27	1 3
18	1 9 22 23	0 42 25	1 5 23	1 4	50 17 23	18 23	1 26 44	1 4
19	1 10 4 48	0 42 23	1 4 25	1 3	50 35 46	18 11	1 27 2	1 5
20	1 10 47 11	0 42 19	1 3 28	1 2	50 53 57	17 59	1 27 19	1 5
21	1 11 29 30	0 42 18	1 2 31	1 0	51 11 56	17 48	1 27 36	1 6
22	1 12 11 48	0 42 15	1 1 34	0 59	51 29 44	17 37	1 27 54	1 7
23	1 12 54 3	0 42 12	1 0 37	0 58	51 47 21	17 24	1 28 12	1 8
24	1 13 36 15	0 42 12	0 59 41	0 57	52 4 45	17 12	1 28 29	1 9
25	1 14 18 27	0 42 10	0 58 41	0 55	52 21 57	17 0	1 28 46	1 10
26	1 15 0 37	0 42 6	0 57 48	0 54	52 38 57	16 47	1 29 4	1 11
27	1 15 42 43	0 42 8	0 56 52	0 53	52 55 44	16 35	1 29 21	1 12
28	1 16 24 51	0 42 4	0 55 56	0 52	53 12 19	16 22	1 29 38	1 12
29	1 17 6 55	0 42 4	0 55 0	0 51	53 28 41	16 9	1 29 55	1 13
30	1 17 48 59	0 42 2	0 54 5	0 49	53 44 50	15 56	1 30 11	1 14
31	1 18 31 1	0 42 3	0 53 10	0 48	54 0 46	15 43	1 30 28	1 15
32	1 19 13 4	0 42 1	0 52 15	0 47	54 16 29	15 30	1 30 45	1 16
33	1 19 55 5	0 42 0	0 51 20	0 46	54 31 59	15 16	1 31 2	1 16
34	1 20 37 5	0 42 1	0 50 25	0 45	54 47 15	15 3	1 31 18	1 17
35	1 21 19 6	0 41 59	0 49 30	0 44	55 2 18	14 49	1 31 34	1 18
36	1 22 1 5	0 41 59	0 48 35	0 43	55 17 7	14 35	1 31 51	1 19
37	1 22 43 4	0 42 0	0 47 41	0 42	55 31 42	14 21	1 32 6	1 19
38	1 23 25 4	0 41 58	0 46 46	0 41	55 46 3	14 8	1 32 22	1 20
39	1 24 7 2	0 41 59	0 45 52	0 40	56 0 11	13 53	1 32 38	1 21
40	1 24 49 1	0 41 59	0 44 58	0 39	56 14 4	13 39	1 32 53	1 22
41	1 25 31 0	0 41 59	0 44 3	0 38	56 27 43	13 24	1 33 8	1 22
42	1 26 12 59	0 42 0	0 43 9	0 37	56 41 7	13 10	1 33 23	1 23
43	1 26 54 59	0 41 59	0 42 14	0 36	56 54 17	12 55	1 33 38	1 24
44	1 27 36 58	0 42 0	0 41 20	0 35	57 7 12	12 41	1 33 53	1 24
45	1 28 18 58	0 42 0	0 40 26	0 34	57 19 53	12 25	1 34 8	1 25
46	1 29 0 58	0 42 0	0 39 33	0 33	57 32 18	12 11	1 34 22	1 26
47	1 29 42 58	0 42 1	0 38 40	0 32	57 44 29	11 55	1 34 36	1 26
48	2 0 24 59	0 42 3	0 37 46	0 31	57 56 24	11 40	1 34 50	1 27
49	2 1 7 2	0 42 2	0 36 52	0 30	58 8 4	11 25	1 35 4	1 28
50	2 1 49 4	0 42 3	0 35 58	0 29	58 19 29	11 9	1 35 17	1 28
51	2 2 31 7	0 42 3	0 35 4	0 28	58 30 38	10 54	1 35 30	1 29
52	2 3 13 10	0 42 4	0 34 11	0 28	58 41 32	10 38	1 35 43	1 29
53	2 3 55 14	0 42 5	0 33 17	0 27	58 52 10	10 22	1 35 56	1 30
54	2 4 37 19	0 42 6	0 32 23	0 26	59 2 32	10 7	1 36 8	1 30
55	2 5 19 25	0 42 6	0 31 29	0 25	59 12 39	9 51	1 36 21	1 31
56	2 6 1 31	0 42 7	0 30 35	0 24	59 22 30	9 34	1 36 33	1 31
57	2 6 43 38	0 42 8	0 29 42	0 23	59 32 4	9 18	1 36 44	1 32
58	2 7 25 46	0 42 10	0 28 48	0 23	59 41 22	9 2	1 36 54	1 32
59	2 8 7 56	0 42 9	0 27 53	0 22	59 50 24	8 46	1 37 5	1 33
60	2 8 50 5		0 27 0	0 21	59 59 10		1 37 16	1 33

The Asc. of M. Cœli.	Longitude of Nonagesima.				Altitude of Nonagesima			
	Longitude.	Diff.	V. for 100′ Var of Lat	V. 100′ V. of Ob	Altitude.	Diff.	Var. for 100′ Var. of Lat	V 100′ V. Ob.
°	s. ° ′ ″	° ′ ″	° ′ ″	′ ″	° ′ ″	′ ″	° ′ ″	′ ″
			+	—			—	—
60	2 8 50 5	0 42 10	0 27 0	0 21	59 59 10	8 30	1 37 16	1 33
61	2 9 32 15	0 42 12	0 26 6	0 20	60 7 40	8 13	1 37 28	1 34
62	2 10 14 27	0 42 11	0 25 12	0 19	60 15 53	7 56	1 37 37	1 34
63	2 10 56 38	0 42 13	0 24 19	0 19	60 23 49	7 39	1 37 47	1 35
64	2 11 38 51	0 42 14	0 23 25	0 18	60 31 28	7 24	1 37 56	1 35
65	2 12 21 5	0 42 15	0 22 31	0 17	60 38 52	7 6	1 38 6	1 35
66	2 13 3 20	0 42 14	0 21 37	0 16	60 45 58	6 49	1 38 14	1 36
67	2 13 45 34	0 42 16	0 20 43	0 16	60 52 47	6 33	1 38 22	1 36
68	2 14 27 50	0 42 17	0 19 50	0 15	60 59 20	6 15	1 38 31	1 36
69	2 15 10 7	0 42 18	0 18 56	0 14	61 5 35	5 58	1 38 38	1 37
70	2 15 52 25	0 42 17	0 18 0	0 13	61 11 33	5 41	1 38 45	1 37
71	2 16 34 42	0 42 19	0 17 8	0 13	61 17 14	5 25	1 38 52	1 37
72	2 17 17 1	0 42 19	0 16 14	0 12	61 22 39	5 7	1 39 0	1 38
73	2 17 59 20	0 42 20	0 15 21	0 11	61 27 46	4 49	1 39 6	1 38
74	2 18 41 40	0 42 21	0 14 26	0 11	61 32 35	4 33	1 39 12	1 38
75	2 19 24 1	0 42 21	0 13 32	0 10	61 37 8	4 15	1 39 18	1 38
76	2 20 6 22	0 42 22	0 12 38	0 9	61 41 23	3 57	1 39 24	1 38
77	2 20 48 44	0 42 22	0 11 44	0 9	61 45 20	3 41	1 39 29	1 39
78	2 21 31 6	0 42 24	0 10 50	0 8	61 49 1	3 22	1 39 33	1 39
79	2 22 13 30	0 42 23	0 9 55	0 7	61 52 23	3 6	1 39 37	1 39
80	2 22 55 53	0 42 23	0 9 1	0 7	61 55 29	2 47	1 39 41	1 39
81	2 23 38 16	0 42 23	0 8 7	0 6	61 58 16	2 31	1 39 45	1 39
82	2 24 20 39	0 42 25	0 7 13	0 5	62 0 47	2 13	1 39 48	1 40
83	2 25 3 4	0 42 25	0 6 29	0 5	62 3 0	1 53	1 39 51	1 40
84	2 25 45 29	0 42 25	0 5 25	0 4	62 4 53	1 37	1 39 53	1 40
85	2 26 27 54	0 42 24	0 4 31	0 3	62 6 30	1 21	1 39 55	1 40
86	2 27 10 18	0 42 26	0 3 37	0 3	62 7 51	1 1	1 39 57	1 40
87	2 27 52 44	0 42 25	0 2 43	0 2	62 8 52	0 45	1 39 58	1 40
88	2 28 35 9	0 42 26	0 1 19	0 1	62 9 37	0 26	1 39 59	1 40
89	2 29 17 35	0 42 25	0 0 51	0 1	62 10 3	0 9	1 40 0	1 40
			+					
90	3 0 0 0	0 42 25	0 0 0	0 0	62 10 12	0 9	1 40 0	1 40
			—	+				
91	3 0 42 25	0 42 26	0 0 [illegible]	0 1	62 10 3	0 26	1 40 0	1 40
92	3 1 24 51	0 42 25	0 1 49	0 1	62 9 37	0 45	1 39 59	1 40
93	3 2 7 16	0 42 26	0 2 43	0 2	62 8 52	1 1	1 39 58	1 40
94	3 2 49 42	0 42 24	0 3 37	0 3	62 7 51	1 21	1 39 57	1 40
95	3 3 32 6	0 42 25	0 4 31	0 3	62 6 30	1 37	1 39 55	1 40
96	3 4 14 31	0 42 25	0 5 25	0 4	62 4 53	1 53	1 39 53	1 40
97	3 4 56 56	0 42 25	0 6 29	0 5	62 3 0	2 13	1 39 51	1 40
98	3 5 39 21	0 42 23	0 7 13	0 5	62 0 47	2 31	1 39 48	1 40
99	3 6 21 44	0 42 23	0 8 7	0 6	61 58 16	2 47	1 39 45	1 39
100	3 7 4 7	0 42 23	0 9 1	0 7	61 55 29	3 6	1 39 41	1 39
101	3 7 46 30	0 42 24	0 9 55	0 7	61 52 23	3 22	1 39 37	1 39
102	3 8 28 51	0 42 22	0 10 50	0 8	61 49 1	3 41	1 39 33	1 39
103	3 9 11 16	0 42 22	0 11 44	0 9	61 45 20	3 57	1 39 29	1 39
104	3 9 53 38	0 42 21	0 12 38	0 9	61 41 23	4 15	1 39 24	1 38
105	3 10 35 59	0 42 21	0 13 32	0 10	61 37 8	4 33	1 39 18	1 38
106	3 11 18 20	0 42 20	0 14 26	0 11	61 32 35	4 49	1 39 12	1 38
107	3 12 0 40	0 42 19	0 15 21	0 11	61 27 46	5 7	1 39 6	1 38
108	3 12 42 59	0 42 19	0 16 14	0 12	61 22 39	5 25	1 39 0	1 38
109	3 13 25 18	0 42 17	0 17 8	0 13	61 17 14	5 41	1 38 52	1 37
110	3 14 7 35	0 42 18	0 18 0	0 13	61 11 33	5 58	1 38 45	1 37
111	3 14 49 53	0 42 17	0 18 56	0 14	61 5 35	6 15	1 38 38	1 37
112	3 15 32 10	0 42 16	0 19 50	0 15	60 59 20	6 33	1 38 31	1 36
113	3 16 14 26	0 42 14	0 20 43	0 16	60 52 47	6 49	1 38 22	1 36
114	3 16 56 40	0 42 15	0 21 37	0 16	60 45 58	7 6	1 38 14	1 36
115	3 17 38 55	0 42 14	0 22 31	0 17	60 38 52	7 24	1 38 6	1 35
116	3 18 21 9	0 42 13	0 23 25	0 18	60 31 28	7 39	1 37 56	1 35
117	3 19 3 22	0 42 11	0 24 19	0 19	60 23 49	7 56	1 37 47	1 35
118	3 19 45 33	0 42 12	0 25 12	0 19	60 15 53	8 13	1 37 37	1 34
119	3 20 27 45	0 42 10	0 26 6	0 20	60 7 40	8 30	1 37 28	1 34
120	3 21 9 55		0 27 0	0 21	59 59 10		1 37 16	1 33

R. A. of M. C.	Longitude of Nonagesima.				Altitude of Nonagesima.			
	Longitude.	Diff.	V for 100′ Var. of Lat	V. 100″ V. of Ob.	Altitude.	Diff.	V. for 100′ Var. of Lat	Var. 10 V. of O
°	s. ° ′ ″	° ′ ″	° ′ ″	′ ″	° ′ ″	′ ″	° ′ ″	′ ″
			—	+			—	—
120	3 21 9 55	0 42 9	0 27 0	0 21	59 59 10	8 46	1 37 16	1 33
121	3 21 52 4	0 42 10	0 27 53	0 22	59 50 24	9 2	1 37 35	1 33
122	3 22 34 14	0 42 8	0 28 48	0 23	59 41 22	9 18	1 36 54	1 32
123	3 23 16 22	0 42 7	0 29 42	0 23	59 32 4	9 34	1 36 44	1 32
124	3 23 58 29	0 42 6	0 30 35	0 24	59 22 30	9 51	1 36 33	1 31
125	3 24 40 35	0 42 6	0 31 29	0 25	59 12 39	10 7	1 36 21	1 31
126	3 25 22 41	0 42 5	0 32 23	0 26	59 2 32	10 22	1 36 8	1 30
127	3 26 4 46	0 42 4	0 33 17	0 27	58 52 10	10 38	1 35 56	1 30
128	3 26 46 50	0 42 3	0 34 11	0 28	58 41 32	10 54	1 35 43	1 29
129	3 27 28 53	0 42 3	0 35 4	0 28	58 30 38	11 9	1 35 30	1 29
130	3 28 10 56	0 42 2	0 35 58	0 29	58 19 29	11 25	1 35 17	1 28
131	3 28 52 58	0 42 3	0 36 52	0 30	58 8 4	11 40	1 35 4	1 28
132	3 29 35 1	0 42 1	0 37 46	0 31	57 56 24	11 55	1 34 50	1 27
133	4 0 17 2	0 42 0	0 38 40	0 32	57 44 29	12 11	1 34 36	1 26
134	4 0 59 2	0 42 0	0 39 33	0 33	57 32 18	12 25	1 34 22	1 26
135	4 1 41 2	0 42 0	0 40 26	0 34	57 19 53	12 41	1 34 8	1 25
136	4 2 23 2	0 41 59	0 41 20	0 35	57 7 12	12 55	1 33 53	1 24
137	4 3 5 1	0 42 0	0 42 14	0 36	56 54 17	13 10	1 33 38	1 24
138	4 3 47 1	0 41 59	0 43 9	0 37	56 41 7	13 24	1 33 23	1 23
139	4 4 29 0	0 41 59	0 44 [illegible]	0 38	56 27 43	13 39	1 33 8	1 22
140	4 5 10 59	0 41 59	0 44 58	0 39	56 14 4	13 53	1 32 53	1 22
141	4 5 52 58	0 41 58	0 45 52	0 40	56 0 11	14 8	1 32 38	1 21
142	4 6 34 56	0 42 0	0 46 46	0 41	55 46 3	14 21	1 32 22	1 20
143	4 7 16 56	0 41 59	0 47 41	0 42	55 31 42	14 35	1 32 6	1 19
144	4 7 58 55	0 41 59	0 48 35	0 43	55 17 7	14 49	1 31 51	1 19
145	4 8 40 54	0 42 1	0 49 30	0 44	55 2 18	15 3	1 31 34	1 18
146	4 9 22 55	0 42 0	0 50 25	0 45	54 47 15	15 16	1 31 18	1 17
147	4 10 4 55	0 42 1	0 51 20	0 46	54 31 59	15 30	1 31 2	1 16
148	4 10 46 56	0 42 3	0 52 15	0 47	54 16 29	15 43	1 30 45	1 16
149	4 11 28 59	0 42 2	0 53 10	0 48	54 0 46	15 56	1 30 28	1 15
150	4 12 11 1	0 42 4	0 54 5	0 49	53 44 50	16 9	1 30 11	1 14
151	4 12 53 5	0 42 4	0 55 0	0 51	53 28 41	16 22	1 29 55	1 13
152	4 13 35 9	0 42 8	0 55 56	0 52	53 12 19	16 35	1 29 38	1 12
153	4 14 17 17	0 42 6	0 56 52	0 53	52 55 44	16 47	1 29 21	1 12
154	4 14 59 23	0 42 10	0 57 48	0 54	52 38 57	17 0	1 29 4	1 11
155	4 15 41 33	0 42 12	0 58 44	0 55	52 21 57	17 12	1 28 46	1 10
156	4 16 23 45	0 42 12	0 59 41	0 57	52 4 45	17 24	1 28 29	1 9
157	4 17 5 57	0 42 15	1 0 37	0 58	51 47 21	17 37	1 28 12	1 8
158	4 17 48 12	0 42 18	1 1 34	0 59	51 29 44	17 48	1 27 54	1 7
159	4 18 30 30	0 42 19	1 2 31	1 0	51 11 56	17 59	1 27 36	1 6
160	4 19 12 49	0 42 23	1 3 28	1 2	50 53 57	18 11	1 27 19	1 5
161	4 19 55 12	0 42 25	1 4 25	1 3	50 35 46	18 23	1 27 2	1 5
162	4 20 37 37	0 42 29	1 5 23	1 4	50 17 23	18 33	1 26 44	1 4
163	4 21 20 6	0 42 31	1 6 21	1 6	49 58 50	18 45	1 26 27	1 3
164	4 22 2 37	0 42 36	1 7 19	1 7	49 40 5	18 56	1 26 9	1 2
165	4 22 45 13	0 42 40	1 8 18	1 8	49 21 9	19 7	1 25 51	1 1
166	4 23 27 53	0 42 43	1 9 17	1 10	49 2 2	19 16	1 25 34	1 0
167	4 24 10 36	0 42 48	1 10 17	1 11	48 42 46	19 28	1 25 17	0 59
168	4 24 53 24	0 42 52	1 11 16	1 13	48 23 18	19 38	1 25 0	0 58
169	4 25 36 16	0 42 57	1 12 16	1 14	48 3 40	19 48	1 24 42	0 57
170	4 26 19 13	0 43 2	1 13 16	1 16	47 43 52	19 58	1 24 25	0 56
171	4 27 2 15	0 43 8	1 14 17	1 17	47 23 54	20 7	1 24 8	0 54
172	4 27 45 23	0 43 13	1 15 17	1 19	47 3 47	20 18	1 23 51	0 53
173	4 28 28 36	0 43 20	1 16 19	1 20	46 43 29	20 27	1 23 34	0 52
174	4 29 11 56	0 43 26	1 17 21	1 22	46 23 2	20 35	1 23 17	0 51
175	4 29 55 22	0 43 33	1 18 24	1 24	46 2 27	20 45	1 23 0	0 50
176	5 0 38 55	0 43 41	1 19 27	1 25	45 41 42	20 54	1 22 44	0 49
177	5 1 22 36	0 43 47	1 20 31	1 27	45 20 48	21 3	1 22 28	0 48
178	5 2 6 23	0 43 56	1 21 35	1 29	44 59 45	21 11	1 22 11	0 47
179	5 2 50 19	0 44 3	1 22 40	1 30	44 38 34	21 20	1 21 55	0 46
180	5 3 34 22		1 23 45	1 32	44 17 14		1 21 39	0 45

M. Cœli.	Longitude of Nonagesima.				Altitude of Nonagesima.			
	Longitude.	Diff.	V. for 100′ V. of Lat	V. 100″ V. of Ob	Altitude.	Diff.	V. for 100″ Var of Lat.	V 100″ V. Ob
°	s ° ′ ″	° ′ ″	° ′ ″	′ ″	° ′ ″	′ ″	° ′ ″	′ ″
			—	+			—	—
180	5 3 34 22	0 44 13	1 23 45	1 32	44 17 14	21 28	1 21 39	0 45
181	5 4 18 35	0 44 21	1 24 50	1 34	43 55 46	21 36	1 21 23	0 43
182	5 5 2 56	0 44 32	1 25 56	1 35	43 34 10	21 43	1 21 7	0 42
183	5 5 47 28	0 44 42	1 27 2	1 37	43 12 27	21 52	1 20 52	0 41
184	5 6 32 10	0 44 52	1 28 10	1 39	42 50 35	21 58	1 20 36	0 40
185	5 7 17 2	0 15 3	1 29 18	1 41	42 28 37	22 7	1 20 22	0 39
186	5 8 2 5	0 45 15	1 30 26	1 43	42 6 30	22 13	1 20 7	0 38
187	5 8 47 20	0 45 27	1 31 36	1 45	41 44 17	22 19	1 19 52	0 37
188	5 9 32 47	0 45 40	1 32 45	1 47	41 21 58	22 27	1 19 38	0 36
189	5 10 18 27	0 45 53	1 33 56	1 48	40 59 31	22 33	1 19 24	0 35
190	5 11 4 20	0 46 6	1 35 7	1 50	40 36 58	22 39	1 19 11	0 34
191	5 11 50 26	0 46 23	1 36 18	1 52	40 14 19	22 45	1 18 58	0 33
192	5 12 36 49	0 46 37	1 37 31	1 54	39 51 34	22 51	1 18 45	0 31
193	5 13 23 26	0 46 52	1 38 45	1 56	39 28 43	22 57	1 18 33	0 29
194	5 14 10 18	0 47 10	1 39 59	1 58	39 5 46	23 1	1 18 20	0 28
195	5 14 57 28	0 47 27	1 41 14	2 0	38 42 45	23 8	1 18 8	0 26
196	5 15 14 55	0 47 45	1 42 30	2 3	38 19 37	23 11	1 17 56	0 25
197	5 16 32 40	0 48 1	1 43 46	2 5	37 56 26	23 17	1 17 46	0 23
198	5 17 20 41	0 48 22	1 45 4	2 7	37 33 9	23 21	1 17 35	0 22
199	5 18 9 7	0 48 45	1 46 22	2 9	37 9 48	23 25	1 17 25	0 21
200	5 18 57 52	0 49 6	1 47 41	2 11	36 46 23	23 28	1 17 16	0 19
201	5 19 46 58	0 49 28	1 49 1	2 14	36 22 55	23 34	1 17 7	0 18
202	5 20 36 26	0 49 53	1 50 21	2 16	35 59 21	23 35	1 16 58	0 16
203	5 21 26 19	0 50 16	1 51 43	2 18	35 35 46	23 39	1 16 50	0 15
204	5 22 16 35	0 50 42	1 53 5	2 21	35 12 7	23 43	1 16 43	0 13
205	5 23 7 17	0 51 10	1 54 28	2 23	34 48 21	23 44	1 16 36	0 12
206	5 23 58 27	0 51 35	1 55 52	2 25	34 24 40	23 46	1 16 30	0 10
207	5 24 50 2	0 52 7	1 57 16	2 28	34 0 54	23 50	1 16 25	0 9
208	5 25 42 9	0 52 36	1 58 42	2 30	33 37 4	23 49	1 16 20	0 7
209	5 26 34 45	0 53 7	2 0 8	2 32	33 13 15	23 52	1 16 16	0 6
210	5 27 27 52	0 53 42	2 1 34	2 35	32 49 23	23 52	1 16 13	0 4
211	5 28 21 34	0 54 14	2 3 3	2 37	32 25 31	23 51	1 16 10	0 3
212	5 29 15 48	0 54 22	2 4 32	2 39	32 1 37	23 53	1 16 8	0 1
								+
213	6 0 10 10	0 55 27	2 6 1	2 42	31 37 44	23 54	1 16 6	0 0
214	6 1 0 7	0 56 9	2 7 31	2 44	31 13 50	23 53	1 16 5	0 2
215	6 2 2 16	0 56 47	2 9 1	2 47	30 49 57	23 53	1 16 6	0 4
216	6 2 59 3	0 57 30	2 10 31	2 49	30 26 4	23 50	1 16 7	0 5
217	6 3 56 33	0 58 16	2 12 1	2 52	30 2 11	23 50	1 16 10	0 7
218	6 4 54 19	0 59 2	2 13 32	2 54	29 38 21	23 40	1 16 15	0 9
219	6 5 53 51	0 59 50	2 15 4	2 57	29 14 38	23 45	1 16 17	0 11
220	6 6 53 11	1 0 11	2 16 36	3 0	28 50 53	23 42	1 16 22	0 12
221	6 7 54 22	1 1 32	2 18 8	3 2	28 27 11	23 38	1 16 28	0 14
222	6 8 55 54	1 2 28	2 19 39	3 5	28 3 33	23 31	1 16 36	0 16
223	6 9 58 22	1 3 24	2 21 9	3 8	27 39 59	23 30	1 16 44	0 18
224	6 11 1 46	1 4 25	2 22 39	3 10	27 16 29	23 24	1 16 53	0 19
225	6 12 6 11	1 5 26	2 24 8	3 13	26 53 5	23 19	1 17 4	0 21
226	6 13 11 37	1 6 31	2 25 36	3 15	26 29 46	23 12	1 17 17	0 23
227	6 14 18 8	1 7 40	2 27 3	3 18	26 6 34	23 6	1 17 30	0 25
228	6 15 25 48	1 8 50	2 28 28	3 20	25 43 28	22 58	1 17 44	0 27
229	6 16 31 58	1 10 2	2 29 52	3 22	25 20 30	22 50	1 18 0	0 29
230	6 17 44 40	1 11 21	2 31 12	3 25	24 57 40	22 40	1 18 18	0 30
231	6 18 56 1	1 12 39	2 32 29	3 27	24 35 0	22 31	1 18 37	0 32
232	6 20 8 40	1 14 1	2 33 43	3 29	24 12 29	22 21	1 18 57	0 35
233	6 21 22 41	1 15 31	2 34 54	3 31	23 50 8	22 9	1 19 18	0 37
234	6 22 38 12	1 16 58	2 36 0	3 33	23 27 59	21 57	1 19 41	0 39
235	6 23 55 10	1 18 32	2 37 1	3 35	23 6 2	21 43	1 20 5	0 41
236	6 25 13 42	1 20 8	2 37 56	3 37	22 44 19	21 30	1 20 31	0 43
237	6 26 33 50	1 21 51	2 38 45	3 39	22 22 49	21 15	1 20 58	0 45
238	6 27 55 41	1 23 36	2 39 27	3 40	22 1 34	20 58	1 21 27	0 47
239	6 29 19 17	1 25 22	2 40 0	3 41	21 40 36	20 41	1 21 58	0 49
240	7 0 41 39		2 40 23	3 41	21 19 55		1 22 30	0 51

R. A. of M. Cæli	Longitude of Nonagesima.				Altitude of Nonagesima.			
	Longitude.	Diff.	V. for 100' Var. of Lat	V. 100" V. of Ob.	Altitude	Diff	V. for 100' Var of Lat	V. 100" V. of Ob
°	s ° ' "	° ' "	° ' "	' "	° ' "	' "	° ' "	' "
			−	+			−	+
240	7 0 44 39	1 27 16	2 40 23	3 41	21 19 55	20 23	1 22 30	0 51
241	7 2 11 55	1 29 12	2 40 38	3 42	20 59 32	20 3	1 23 3	0 54
242	7 3 41 7	1 31 11	2 40 38	3 41	20 39 29	19 42	1 23 39	0 56
243	7 5 12 18	1 33 16	2 40 27	3 41	20 19 47	19 20	1 24 15	0 58
244	7 6 45 34	1 35 25	2 40 3	3 40	20 0 27	18 57	1 24 53	1 0
245	7 8 20 59	1 37 34	2 39 26	3 39	19 41 30	18 31	1 25 32	1 2
246	7 9 58 33	1 39 44	2 38 27	3 37	19 22 59	18 6	1 26 13	1 4
247	7 11 38 17	1 42 7	2 37 9	3 35	19 4 53	17 36	1 26 55	1 7
248	7 13 20 24	1 44 25	2 35 33	3 32	18 47 17	17 9	1 27 38	1 9
249	7 15 4 49	1 46 47	2 33 34	3 30	18 30 8	16 36	1 28 22	1 11
250	7 16 51 36	1 49 9	2 31 15	3 28	18 13 32	16 3	1 29 6	1 13
251	7 18 40 45	1 51 39	2 28 24	3 24	17 57 29	15 29	1 29 52	1 15
252	7 20 32 24	1 54 3	2 25 12	3 19	17 42 0	14 53	1 30 38	1 17
253	7 22 26 27	1 56 30	2 21 24	3 14	17 27 7	14 14	1 31 24	1 19
254	7 24 22 57	1 58 56	2 17 14	3 8	17 12 53	13 35	1 32 10	1 21
255	7 26 21 53	2 1 23	2 12 29	3 1	16 59 18	12 53	1 32 56	1 23
256	7 28 23 16	2 3 41	2 7 13	2 54	16 46 25	12 9	1 33 41	1 25
257	8 0 26 57	2 6 3	2 1 22	2 46	16 34 16	11 24	1 34 25	1 27
258	8 2 33 0	2 8 18	1 54 57	2 37	16 22 52	10 38	1 35 7	1 29
259	8 4 41 18	2 10 26	1 47 57	2 27	16 12 14	9 47	1 35 48	1 31
260	8 6 51 44	2 12 27	1 40 24	2 17	16 2 27	8 58	1 36 29	1 32
261	8 9 4 11	2 14 18	1 32 17	2 6	15 53 29	8 6	1 37 5	1 33
262	8 11 18 29	2 16 0	1 23 35	1 54	15 45 23	7 12	1 37 40	1 35
263	8 13 34 38	2 17 40	1 13 58	1 41	15 38 11	6 19	1 38 11	1 36
264	8 15 52 18	2 19 5	1 4 16	1 28	15 31 52	5 22	1 38 38	1 37
265	8 18 11 23	2 20 14	0 54 41	1 14	15 26 30	4 24	1 39 2	1 38
266	8 20 31 37	2 21 15	0 44 12	1 0	15 22 6	3 27	1 39 23	1 39
267	8 22 52 52	2 21 56	0 33 26	0 45	15 18 39	2 28	1 39 39	1 40
268	8 25 14 48	2 22 29	0 22 21	0 30	15 16 11	1 29	1 39 50	1 40
269	8 27 37 17	2 22 43	0 11 11	0 15	15 14 42	0 30	1 39 55	1 40
					15 14 12		1 40 0	1 40
270	9 0 0 0	2 22 43	0 0 0	0 0	—	0 30	—	—
			+	−				
271	9 2 22 43	2 22 29	0 11 11	0 15	15 14 42	1 29	1 39 55	1 40
272	9 4 45 12	2 21 56	0 22 21	0 30	15 16 11	2 28	1 39 50	1 40
273	9 7 7 8	2 21 15	0 33 26	0 45	15 18 39	3 27	1 39 39	1 40
274	9 9 28 23	2 20 14	0 44 12	1 0	15 22 6	4 24	1 39 23	1 39
275	9 11 48 37	2 19 5	0 54 41	1 14	15 26 30	5 22	1 39 2	1 38
276	9 14 7 42	2 17 40	1 4 46	1 28	15 31 52	6 19	1 38 38	1 37
277	9 16 25 22	2 16 9	1 13 58	1 41	15 38 11	7 12	1 38 11	1 36
278	9 18 41 31	2 14 18	1 23 35	1 54	15 45 23	8 6	1 37 40	1 35
279	9 20 55 49	2 12 27	1 32 17	2 6	15 53 29	8 58	1 37 5	1 33
280	9 23 8 16	2 10 26	1 40 24	2 17	16 2 27	9 47	1 36 29	1 32
281	9 25 18 42	2 8 18	1 47 57	2 27	16 12 14	10 38	1 35 48	1 31
282	9 27 27 0	2 6 3	1 54 57	2 37	16 22 52	11 24	1 35 7	1 29
283	9 29 33 3	2 3 41	2 1 22	2 46	16 34 16	12 9	1 34 25	1 27
284	10 1 36 44	2 1 23	2 7 13	2 54	16 46 25	12 53	1 33 41	1 25
285	10 3 38 7	1 58 56	2 12 29	3 1	16 59 18	13 35	1 32 55	1 23
286	10 5 37 3	1 56 30	2 17 14	3 8	17 12 53	14 14	1 32 10	1 21
287	10 7 33 33	1 54 3	2 21 24	3 14	17 27 7	14 53	1 31 24	1 19
288	10 9 27 36	1 51 39	2 25 12	3 19	17 42 0	15 29	1 30 38	1 17
289	10 11 19 15	1 49 9	2 28 24	3 24	17 57 29	16 3	1 29 52	1 15
290	10 13 8 24	1 46 47	2 31 15	3 28	18 13 32	16 36	1 29 6	1 13
291	10 14 55 11	1 44 25	2 33 34	3 30	18 30 8	17 9	1 28 22	1 11
292	10 16 39 36	1 42 7	2 35 33	3 32	18 47 17	17 36	1 27 38	1 9
293	10 18 21 43	1 39 44	2 37 9	3 35	19 4 53	18 6	1 26 55	1 7
294	10 20 1 27	1 37 34	2 38 27	3 37	19 22 59	18 31	1 26 13	1 4
295	10 21 39 1	1 35 25	2 39 26	3 39	19 41 30	18 57	1 25 32	1 2
296	10 23 14 26	1 33 16	2 40 3	3 40	20 0 27	19 20	1 24 53	1 0
297	10 24 47 42	1 31 11	2 40 27	3 41	20 19 47	19 42	1 24 15	0 58
298	10 26 18 53		2 40 38	3 41	20 39 29		1 23 39	0 56

R.A. of M.Cœli	Longitude of Nonagesima.					Altitude of Nonagesima.			
	Longitude.	Diff	V. for 100′ Var. of Lat	V. 100″ V. of Ob.		Altitude.	Diff.	V. for 100′ Var. of Lat	Var 100″ V of Ob
°	s. ° ′ ″	° ′ ″	° ′ ″	′ ″		° ′ ″	′ ″	° ′ ″	′ ″
			+	−				−	+
300	10 29 15 21	1 25 22	2 40 23	3 41		21 19 55	20 41	1 22 30	0 51
301	11 0 40 43	1 23 36	2 40 0	3 41		21 40 36	20 58	1 21 58	0 49
302	11 2 4 19	1 21 51	2 39 27	3 40		22 1 34	21 15	1 21 27	0 47
303	11 3 26 10	1 20 8	2 38 45	3 39		22 22 49	21 30	1 20 58	0 45
304	11 4 46 18	1 18 32	2 37 56	3 37		22 44 19	21 43	1 20 31	0 43
305	11 6 4 50	1 16 58	2 37 1	3 35		23 6 2	21 57	1 20 5	0 41
306	11 7 21 48	1 15 31	2 36 0	3 33		23 27 59	22 9	1 19 41	0 39
307	11 8 37 19	1 14 1	2 34 51	3 31		23 50 8	22 21	1 19 18	0 37
308	11 9 51 20	1 12 39	2 33 43	3 29		24 12 29	22 31	1 18 57	0 35
309	11 11 3 59	1 11 21	2 32 29	3 27		24 35 0	22 40	1 18 37	0 32
310	11 12 15 20	1 10 2	2 31 12	3 25		24 57 40	22 50	1 18 18	0 30
311	11 13 25 22	1 8 50	2 29 52	3 22		25 20 30	22 58	1 18 0	0 29
312	11 14 34 12	1 7 40	2 28 28	3 20		25 43 28	23 6	1 17 44	0 27
313	11 15 41 52	1 6 31	2 27 3	3 18		26 6 34	23 12	1 17 30	0 25
314	11 16 48 23	1 5 26	2 25 36	3 15		26 29 46	23 19	1 17 17	0 23
315	11 17 53 49	1 4 25	2 24 8	3 13		26 53 5	23 24	1 17 4	0 21
316	11 18 58 14	1 3 24	2 22 39	3 10		27 16 29	23 30	1 16 53	0 19
317	11 20 1 38	1 2 28	2 21 9	3 8		27 39 59	23 34	1 16 44	0 18
318	11 21 4 6	1 1 32	2 19 39	3 5		28 3 33	23 38	1 16 36	0 16
319	11 22 5 38	1 0 41	2 18 8	3 2		28 27 11	23 42	1 16 28	0 14
320	11 23 6 19	0 59 50	2 16 36	3 0		28 50 53	23 45	1 16 22	0 12
321	11 24 6 9	0 59 2	2 15 4	2 57		29 14 38	23 46	1 16 17	0 11
322	11 25 5 11	0 58 16	2 13 33	2 54		29 38 21	23 50	1 16 13	0 9
323	11 26 3 27	0 57 30	2 12 1	2 52		30 2 11	23 50	1 16 10	0 7
324	11 27 0 57	0 56 47	2 10 31	2 49		30 26 4	23 53	1 16 7	0 5
325	11 27 57 44	0 56 9	2 9 1	2 47		30 49 57	23 54	1 16 6	0 4
326	11 28 53 53	0 55 27	2 7 31	2 41		31 13 50	23 53	1 16 5	0 2
327	11 29 49 20	0 54 52	2 6 1	2 42		31 37 44	23 54	1 16 6	0 0
									−
328	0 0 44 12	0 54 14	2 4 32	2 39		32 1 37	23 52	1 16 8	0 1
329	0 1 38 26	0 53 42	2 3 3	2 37		32 25 31	23 52	1 16 10	0 3
330	0 2 32 8	0 53 7	2 1 34	2 35		32 49 23	23 49	1 16 13	0 4
331	0 3 25 15	0 52 36	2 0 8	2 32		33 13 15	23 50	1 16 16	0 6
332	0 4 17 51	0 52 7	1 58 42	2 30		33 37 4	23 46	1 16 20	0 7
333	0 5 9 58	0 51 35	1 57 16	2 28		34 0 54	23 44	1 16 25	0 9
334	0 6 1 33	0 51 10	1 55 52	2 25		34 24 40	23 43	1 16 30	0 10
335	0 6 52 43	0 50 42	1 54 28	2 23		34 48 24	23 39	1 16 36	0 12
336	0 7 43 25	0 50 16	1 53 5	2 21		35 12 7	23 35	1 16 43	0 13
337	0 8 33 41	0 49 53	1 51 43	2 18		35 35 46	23 34	1 16 50	0 15
338	0 9 23 34	0 49 28	1 50 21	2 16		35 59 21	23 28	1 16 58	0 16
339	0 10 13 2	0 49 6	1 49 1	2 14		36 22 55	23 25	1 17 7	0 18
340	0 11 2 8	0 48 45	1 47 41	2 11		36 46 23	23 21	1 17 16	0 19
341	0 11 50 53	0 48 23	1 46 22	2 9		37 9 48	23 17	1 17 25	0 21
342	0 12 39 16	0 48 4	1 45 4	2 7		37 33 9	23 11	1 17 35	0 22
343	0 13 27 20	0 47 45	1 43 46	2 5		37 56 26	23 8	1 17 46	0 23
344	0 14 15 5	0 47 27	1 42 30	2 3		38 19 37	23 1	1 17 56	0 25
345	0 15 2 32	0 47 10	1 41 14	2 0		38 42 45	22 57	1 18 8	0 26
346	0 15 49 42	0 46 52	1 39 59	1 58		39 5 46	22 51	1 18 20	0 28
347	0 16 36 34	0 46 37	1 38 45	1 56		39 28 43	22 45	1 18 33	0 29
348	0 17 23 11	0 46 23	1 37 31	1 54		39 51 34	22 39	1 18 45	0 31
349	0 18 9 34	0 46 6	1 36 18	1 52		40 14 19	22 33	1 18 58	0 33
350	0 18 55 40	0 45 53	1 35 7	1 50		40 36 58	22 27	1 19 11	0 34
351	0 19 41 33	0 45 40	1 33 56	1 48		40 59 31	22 19	1 19 24	0 35
352	0 20 27 13	0 45 27	1 32 45	1 47		41 21 58	22 13	1 19 38	0 36
353	0 21 12 40	0 45 15	1 31 36	1 45		41 44 17	22 7	1 19 52	0 37
354	0 21 57 55	0 45 3	1 30 26	1 43		42 6 30	21 58	1 20 7	0 38
355	0 22 42 58	0 44 52	1 29 18	1 41		42 28 37	21 52	1 20 22	0 39
356	0 23 27 50	0 44 42	1 28 10	1 39		42 50 35	21 43	1 20 36	0 40
357	0 24 12 32	0 44 32	1 27 2	1 37		43 12 27	21 36	1 20 52	0 41
358	0 24 57 4	0 44 21	1 25 56	1 35		43 34 10	21 28	1 21 7	0 42
359	0 25 41 25		1 24 50	1 34		43 55 16		1 21 23	0 43

Note.—If the Latitude be South of Greenwich, or the Obliquity of the Ecliptic be greater than 23° 28′, change the signs in the Table, and apply the variations accordingly.

The above Table was calculated by the formula of Dr. Brinkley at the Observatory of Trinity College, Dublin; viz. Let L = the latitude reduced; O = Ob. eclip.; A = A.R. of Medium Cœli; Then cos. A + cos. L = cos. arc I. which is greater than a quadrant in the second and third quadrants of *Med. Cœli.* Cot. L + sin A = arc II. which is always less than a quadrant. Arc II. ± O = arc III. where — takes place when Aries is West of the meridian, and + when East. Cos. of *alt. nonag.* = sin arc I. + sin arc III. Tang. *long. nonag.* = cos arc III. + tang. arc I. When arc III. is *less* than a quadrant, the *long. nonag.* is of the same affection as A; when *greater*, of the same same affection as arc I.

The *long.* and *alt.* of the *nonag.* was calculated for the latitude of Greenwich, and likewise for 100 minutes of lat. N. of Greenwich, and the variations obtained by taking the difference.

XLIV. *True apparent Right Ascension of* Dr. MASKELYNE's 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. GROOBY.

[Continued from p. 112.]

To Dr. Tilloch.

SIR, — I BEG leave to apprize those of your readers who may have occasion to use the following Tables, that for the months of Nov. and Dec. the Right Ascensions are calculated from the Tables of M. Bessel, annexed to the first part of the Astronomical Observations at the Royal Observatory in Konigsberg, and that they give the apparent Right Ascension of the stars *at the time of their culmination*, and *not* at the beginning of the day.

It is needless for me to make any observation on the utility of this alteration which the learned Professor has made in the construction of his Tables, or on their acknowledged superiority, in point of accuracy, over any other tables of this kind that have yet been published. Had I seen them before I had made the calculations for the former months, I should have used no other. Those days in which any of the stars pass the meridian twice, are distinguished by an asterisk, and the right ascension in such case is that at the first passage.

I am, sir,
Your obedient servant,
Cirencester, Sept. 15, 1821. JAMES GROOBY.

1821	γ Pegasi	α Arietis	α Ceti	Aldebaran	Capella	Rigel	β Tauri	α Orionis	Sirius	Castor	Procyon	Pollux	α Hydrae	Regulus	β Leonis	β Virginis	Spica Virginis	Arcturus
	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M
	0 4	1 57	2 53	4 25	5 3	5 6	5 15	5 45	6 37	7 23	7 29	7 34	9 15	9 58	11 39	11 41	13 15	14 7
Nov.	S	S	S	S	S	S	S	S	S	S	S	S	S	S	S	S	S	S
1	5 84	10 83	0 39	41 26	32 75	0 39	4 05	33 31	18 82	14 45	59 41	25 39	50 25	52 86	57 81	24 69	45 41	31 71
2	83	84	40	28	79	41	08	33	85	49	44	43	28	89	83	72	43	73
3	83	85	41	30	82	43	11	37	88	52	47	47	32	92	86	74	45	75
4	82	86	42	32	86	46	14	40	91	57	50	50	35	95	88	77	47	77
5	82	87	43	34	89	48	16	42	94	60	53	54	38	98	91	79	48	78
6	81	87	44	36	93	50	19	45	97	64	56	57	41	53 01	94	82	50	79
7	80	88	45	38	96	52	21	47	19 00	67	59	61	44	04	97	85	52	81
8	80	88	45	40	99	54	24	50	02	71	62	65	47	07	58 00	87	54	82
9	79	89	46	42	33 01	56	26	52	05	74	65	68	50	10	02	90	56	83
10	79	89	47	44	04	58	29	55	08	78	68	72	53	13	05	93	58	85
11	78	89	47	46	07	60	31	57	10	81	71	75	56	16	08	95	60	86
12	78	89	48	48	10	62	34	59	13	85	74	79	60	20	11	98	62	87
13	77	89	48	50	13	64	36	62	15	88	77	82	63	23	14	25 01	65	89
14	77	90	49	52	16	66	39	64	18	92	80	86	66	26	17	04	67	90
15	76	90	49	53	18	68	41	66	21	95	83	89	70	29	20	07	70	91
16	75	90	50	55	21	69	44	68	24	99	86	93	73	33	23	09	73	93
17	75	90	50	57	23	71	46	70	27	15 02	89	97	76	36	26	12	75	95
18	74	90	51	58	26	73	48	72	29	05	92	26 00	79	39	29	15	78	96
19	73	90	51	60	29	75	51	75	32	09	94	03	83	43	32	18	80	98
20	72	91	52	61	31	76	53	77	34	12	97	06	86	46	35	21	82	32 00
21	71	91	53	63	34	78	55	79	37	16	60 00	10	89	49	38	24	[illegible]	02
22	71	91	53	64	37	80	57	81	39	19	03	13	92	52	41	27	[illegible]	04
23	70	91	54	66	39	82	60	84	42	22	06	17	95	55	44	[illegible]	[illegible]	06
24	69	91	55	68	42	84	62	86	44	26	09	20	99	59	47	[illegible]	92	08
25	68	90	55	69	44	85	64	88	47	29	12	23	51 02	62	50	[illegible]	[illegible]	10
26	67	90	55	70	46	86	66	90	49	32	14	26	05	66	[illegible]	40	[illegible]	12
27	66	90	56	71	48	88	68	92	51	35	17	29	09	69	[illegible]	43	46 00	15
28	65	90	56	72	50	89	70	94	53	39	20	32	12	72	[illegible]	46	03	17
29	64	90	56	74	53	91	72	95	55	42	23	36	15	76	[illegible]	[illegible]	05	19
30	63	90	56	75	55	92	73	97	57	45	25	39	18	79	65	53	[illegible]	21

Days.	[illegible]	α² Libræ	α Coronæ Bor.	α Serpentis	Antares	α Herculis	α Ophiuchi	α Lyræ	γ Aquilæ	α Aquilæ	β Aquilæ	α¹ Capri.	α² Capri.	α Cygni	α Aquarii	Fomalhaut	α Pegasi	α Andromedæ
	H M 11 40	H M 14 41	H M 15 27	H M 15 35	H M 16 18	H M 17 6	H M 17 25	H M 18 30	H M 19 37	H M 19 42	H M 19 46	H M 20 7	H M 20 8	H M 20 34	H M 21 56	H M 22 47	H M 22 55	H M 23 59
	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s
1	30 14	1 58	8 11	29 31	29 09	31 22	30 70	33 97	47 52	5 51	33 91	46 35	10 11	21 68	38 80	48 66	54 74	13 27
2	15	59	11	31	09	21	69	95	50	53	90	34	10	65	78	65	73	26
3	16	60	12	31	09	20	68	93	49	51	89	32	08	63	77	64	72	25
4	17	61	13	31	09	20	67	91	48	50	87	31	07	60	76	63	71	25
5	19	63	14	31	09	20	66	89	47	49	86	30	06	58	74	62	70	24
6	20	64	14	32	09	19	65	87	45	48	84	29	05	55	73	61	69	24
7	21	65	15	32	09	19	65	85	44	47	83	28	04	53	72	60	68	23
8	22	67	15	32	09	18	64	84	43	46	82	27	03	51	71	59	67	22
9	23	68	16	33	09	18	64	82	42	45	81	26	02	49	70	57	65	21
10	25	69	17	33	09	17	63	80	41	44	80	25	01	46	69	55	64	20
11	26	70	17	34	10	17	63	79	40	43	79	24	00	44	68	54	63	19
12	28	72	17	35	10	16	62	77	39	42	78	23	9 99	42	66	53	62	18
13	29	73	18	35	10	16	62	75	38	41	77	22	98	39	65	52	61	18
14	30	74	18	36	11	16	61	73	37	40	76	21	97	37	64	51	60	17
15	32	76	19	37	11	15	61	72	36	39	75	20	96	34	63	49	59	16
16	33	78	20	38	12	15	61	70	35	38	74	19	95	32	62	47	58	15
17	35	79	22	39	13	15	61	69	34	37	73	18	94	30	60	46	57	14
18	37	81	23	40	13	15	61	68	33	36	72	17	93	27	59	44	56	13
19	38	83	24	42	14	15	61	67	32	35	71	16	92	25	58	43	55	12
20	40	85	25	43	15	15	60	66	31	34	70	15	91	23	57	41	53	11
21	42	86	26	44	16	15	60	64	30	33	69	14	90	21	56	40	52	10
22	44	88	27	45	17	15	60	63	29	32	68	13	89	19	54	38	51	09
23	45	90	28	46	18	16	60	62	28	31	67	12	88	16	53	37	50	08
24	47	92	29	47	19	16	60	61	27	30	66	11	87	14	52	35	49	07
25	49	94	30	49	20	16	60	60	26	30	65	10	86	12	51	34	48	06
26	52	96	32	50	21	17	60	59	25	29	64	09	85	10	50	32	47	05
27	54	98	33	52	23	17	61	58	25	28	64	08	84	08	49	31	46	03
28	56	00	35	53	25	17	61	58	24	28	63	07	84	06	47	30	45	02
29	58	02	36	55	26	18	62	57	23	27	62	06	83	04	46	28	43	01
30	00	05	38	56	28	18	62	56	22	26	62	06	82	02	45	27	42	00

XLV. On

XLV. *On the Appearance of Meteors, Parhelia, and Paraselenæ, as Prognostics in general of Wind and Rain.* By *Dr.* W. BURNEY.

Gosport Observatory, Sept 19, 1821.

SIR, — ACCORDING to promise I herewith send you a few more observations on Meteors, since the 12th ult. as a continuation of my article on that subject in your last Number, and am

Yours truly,

To Dr. Tilloch. WILLIAM BURNEY.

August 18, 1821. At 10 minutes before 10 P M. a coloured meteor passed from the star ζ in Aquila to α in the head of Hercules, a space of 26°: the train was of a light red colour and about 16° long. At 35 minutes past 10, a bright meteor, without a train, appeared about 12° above the western point of the horizon, and descended obliquely towards the S.W. Copious dews fell in this and the subsequent night.

—— 20th. Two small meteors appeared at a quarter before 9 P M., one on each side of the Northern Crown.

—— 21st. From 9 till 12 P.M. nine small meteors appeared in various directions in an apparently clear sky, one of which had a train behind it.

—— 22d. Between 9 and 10 P.M. three small meteors appeared to the westward.

—— 23d. About 9 P.M. a large and brilliant meteor with a long coloured train appeared several seconds in descending obliquely from near the zenith towards the N.W.: seven other meteors appeared in various parts of the sky between 9 and 12 o'clock, with no other characteristic than that of being small, lofty, and having a great velocity.

—— 24th. From 10 till 1 P.M four small meteors appeared, two under the constellation Hercules, one under Ursa Major, and one under Georgium Sidus. The three preceding days were the hottest of all others in this month; and from this time till the 27th inclusive, the sky was filled with vapours, followed by 2½ inches of rain by the end of the month, generally accompanied by strong easterly gales.

Sept. 1st. Three small meteors descended between the clouds in a westerly direction, about 11 P.M. On the 3d two winds crossed each other at right angles, and the lower current from the S.W. terminated in a brisk gale on the 4th.

—— 6th. At 6 A.M. two beautifully coloured *parhelia* were observed here, one on each side of, and both 22° 35′ distant from, the sun, which was then due East. The silvery colour behind the red portion of the parhelion to the North of the sun, was so brilliant as scarcely to be viewed with the naked eye, and appeared to be formed in a mixture of cirrocumulative and cirrostrative clouds; the parhelion to the South of the sun was formed last, and both entirely disappeared when the clouds passed off. These mock-suns were followed by a faint solar *halo*, two winds, the lower one from S.E. and the upper one from S.W., and frequent showers in the day. Vivid lightning and distant thunder prevailed throughout the night. From 12 till 2 o'clock, after a heavy shower of rain, the warm flashes of lightning were awfully grand, particularly those in the northern part of the horizon, where the electric streams frequently shot up from the clouds 3° or 4° in perpendicular and oblique directions.

Sept. 9th. At a quarter past 8 P.M. a coloured meteor with a short train descended almost perpendicularly from behind a large cloud, and appeared to fall in the western point of the horizon. A very stormy night followed.

—— 10th. At half past 7 A.M. a bright parhelion appeared to the North of, and 22° 40′ radius from, the sun; and at 8 o'clock a perfect rainbow; also *two others*, and a solar *halo*, in the course of the day. Between 7 and 8 P.M. two faint *paraselenæ* appeared, one on each side of the moon at the exterior edge of a large *halo*, on the top of which a small inverted arc tended to create another paraselene: each of them was 22° 45′ distant from the moon, and situated in cirrostrative vapour.

—— 11th. Between 8 and 9 P.M. three coloured *paraselenæ* appeared in cirrostrative beds of vapour, one on each side of the moon; the other at the top of a large *halo* that surrounded her, and all of them were nearly of the same radius as those last evening: after these rare *phænomena* had disappeared, the moon was apparently encompassed by a close yellow *corona*, and a green circle 1½° in diameter, followed in the night by heavy showers and a gale from S.W.: this change was previously indicated by the sinking of the barometer in the afternoon. The 12th, 13th, and 14th, were distinguished by wind and rain.

—— 18th. At 10 minutes past 8 P.M. a meteor with a sparkling train appeared between the stars Alamak in Andromeda, and Algol in Medusa's head; between that time and ten o'clock, nine other meteors were seen without trains, five towards the East, three to the South, and one in a Westerly direction. The heavenly concave and the Milky Way at this time appeared in their most refulgent splendour, thickly studded with stars, and the wind freshening. A gale sprung up from the N.W. in the night, and prevailed the next day from the same quarter.

It is hoped that these observations will be deemed sufficient to establish a received opinion, That meteors are in general prognostics of wind, or wind and rain.

XLVI. *On Mr.* RIDDLE's *Claim to the Invention of a new Method of determining the Latitude. By Mr.* HENRY ATKINSON.

To Dr. Tilloch.

SIR, — IN perusing the Number of the Philosophical Magazine for July, my attention was arrested by an article entitled "Remarks on Mr. Riddle's Claim to the Invention of a new Method of determining the Latitude." As I happen to be acquainted with some circumstances connected with the insinuation thrown out by the writer, that Mr. Riddle obtained the first idea of the method of calculation under discussion from General B.'s paper, which γ asserts was published "before Mr. R. had said any thing about it, and the memoirs which followed were merely a continuation of the same or a somewhat similar method:"— With respect to the former part of the assertion, I feel myself called upon, in justice to a most worthy and honourable individual, publicly to declare, that *to my certain knowledge* Mr. Riddle had practised the method of determining the latitude described in his paper of October 21, 1818, as well as that given by General B. for determining the time with accuracy, dated "Paris, 23d November 1817," *previous to the period when this latter paper was written:* and, from various circumstances, I have every reason to believe that he had practised them for some years before the period to which I can speak from my own knowledge.

I have now stated the principal cause of my addressing you on this subject; yet in addition, will you permit me, sir, to observe, that when Mr. Riddle found that General B. had laid a method of determining the latitude by a sextant or circle before so learned a body of men as the Royal Society of Edinburgh, without the slightest hint that it had ever been published before; that this memoir was one selected for publication in the Transactions of the Society, without any notification that it was not new; I do not see how Mr. Riddle, knowing that he had published the same thing two years before, could well say less than he did: nor would it have been calculated to excite any great degree of surprise, had he claimed it, "as a discovery," in much stronger language than by simply saying, "The method of General B. is even more like mine than I was likely to anticipate." But if it be true, as γ asserts, that General B. received it "from the continental observers," his conduct in publishing it as he has done is by no means calculated to do him honour: neither will its appearance in the printed Transactions of the Royal Society of Edinburgh be very creditable to that body, if the method be no way different from that which γ says is described at length in the writings of three different foreigners.

Yours very respectfully,

Newcastle, Aug. 11, 1821. HENRY ATKINSON.

XLVII. *On Mr.* PERKINS'S *Conclusions with regard to the Compressibility of Water, drawn from the Results of empty Bottles sunk to different Depths in the Ocean. By Mr.* JOHN DEUCHAR, *M.W.S., Lecturer on Chemistry and on Materia Medica and Pharmacy in Edinburgh.*

To Dr. Tilloch.

DEAR SIR,—MY attention was some years ago directed to the porous nature of glass, with the hope of ascertaining its extent, and how it might be assisted by pressure: and in May last I collected together the result of my observations on the subject, and laid them before the Wernerian Society. In prosecuting this subject I was led to examine every properly authenticated account of bottles filled only with atmospheric air, which, although properly secured at the mouth, after being sunk to a considerable depth in the sea had been brought up full of water. The most recent experiments of which I could obtain an account, were those of Mr. Perkins, contained in a paper upon the Compressibility of Water, read before the Royal Society of London, and inserted in their Transactions for 1820, Part II. Though I differ from Mr. Perkins in my account of the manner in which the water gets into the bottles; yet I do not mean at present to enter upon that part of the subject, as I have discussed it fully in the paper above alluded to, which will be immediately published in the Transactions of the Wernerian Society; I intend to confine myself to a remark or two upon the intention with which these bottles were sunk by Mr. Perkins, with the view of suggesting a more advisable mode of performing the experiment for the purpose of proving what he wished.

The bottles sunk by Mr. Perkins, besides being well corked, were generally secured by six layers of cotton or linen cloth, saturated in a composition of tar and wax. The first experiment from which he draws any conclusion in favour of his hypothesis, is the third (see Phil. Mag. vol. lvii. page 54): the bottle was sunk 300 fathoms; when drawn up, only a part of the neck remained attached to the line. He concludes that the result was not from external pressure, but from the expansion of the condensed sea water in the bottle; because the cork was compressed into half its length, making folds of about 1-8th of an inch; and because the coverings, consisting of six layers of cloth and cement, had been torn up on one side. Now, from these circumstances, he was not entitled to draw the above conclusion; nor, supposing he had proved the pressure from without to have had no concern in producing the effect, was he entitled to ascribe it to the expansion of the water. The great compressibility of air, convinces us that little resistance was to be looked for from it to the external pressure of the water: the whole must have therefore depended upon the strength of the glass and coverings. Now the failure of either of these might be the destruction of the other by the force with which the water would enter, similar to the accidents which sometimes occur, when we suddenly cut a piece of bladder tied over the top of an exhausted receiver, the glass of which is rather thinner than usual. Should therefore the compactness of the glass and the closeness of the coverings resist the entrance of the water, under so great a pressure from without, we could expect nothing else but that the coverings should be torn, the bottle broken, and the cork probably compressed: or, if we suppose the concave bottom of the bottle to have given way, then the rush of water upwards, into what we might under that pressure, comparatively speaking, call a vacuum, would be powerful, and may be supposed to have compressed the cork.

But, in the second place, let us for a moment allow with Mr. Perkins, that the external pressure did not produce the result; it by no means follows of necessity that the water had been compressed, and that it had burst the glass by resuming its former volume, when drawn to the surface. The bottles at the time they were sent down were filled with air; when the water therefore enters, the air must be absorbed, and this absorption can only be maintained by continuing the external pressure: now when we draw up the bottle, the water and air will have a tendency to separate, and, as the space it formerly occupied is filled with water, the elastic force with which it must act will be very great; and this accounts for the bottles only coming up whole when a space was left at the top by the water, to receive the compressed air. In confirmation so far of this, Mr. Perkins remarks, that when the water was poured out it effervesced like mineral water.

Thus it would appear that the mode adopted by Mr. Perkins in these experiments, was rather inadequate to the purpose he had in view. I would therefore, in conclusion, suggest, that in any future trials he may be disposed to make, he should previously fill the bottles to the bottom of the corks with water, that the whole of the air may be removed: and, to render the result still more conclusive, the bottles ought to be inclosed in a cage of iron or copper. After these precautions, should he find that the bottles were broken when brought up, he might then justly conclude, that the re-expansion of the compressed water had been the cause, since the counter-resistance of the water within, must have prevented the effect of pressure from without so considerably as to prevent the breaking of the bottles from that direction: the only way, therefore, in which they could be broken, would be by the external column of water compressing the confined portion, and forcing an additional quantity into the interior: and by this again resuming its former volume when the acting pressure is diminished. I remain, dear sir,

Yours respectfully,

John Deuchar.

XLVIII.—*New Determination of the Proportions of the Constituents of Water; and the Density of certain Elastic Fluids. By MM.* Berzelius *and* Dulong [*].

As modern chemists in their analyses claim a degree of accuracy embracing the thousandth part of the elements employed, it is evident that the fundamental data from which they make their deductions should be free from that degree of error which they profess themselves able to avoid in their experiments. Of these data, that of the constitution of water is one of the most important, and the most frequently employed. The proportions generally adopted of late years have appeared to be beyond suspicion of error, both by the means employed to obtain them, and the ability of the observers who have conducted them. But we have had some reason to believe that this number was liable to be affected by some slight error; and as the subject was of considerable importance, we resolved to conduct, in concert, the experiments necessary to ascertain this point. M. Berthollet, whose liberality has so often been useful to science, gave us every possible facility in our design, by putting at our disposal the laboratory of Arcueil.

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[*] From the *Annales de Chimie et de Physique.*

The apparatus employed in the first experiments on the composition of water, did not allow of that precision which is now required in chemical analysis. But the fact being once established, that water is the result of the combination of oxygen and hydrogen, the knowledge of the precise proportions required only two facts to be determined; namely, the relative volumes of the two elements, and their specific gravities. The latter, being indispensable in a variety of researches, were already known; and for the former, Volta's eudiometer was sufficient. The greatest confidence was justly reposed on this method, after Messrs. Gay-Lussac and Humboldt had shown, in their masterly Memoir on Eudiometry, the true proportions, in volume, of the constituents of water; and after Messrs. Biot and Arrago had applied the most minute attention to the determination of the specific gravity of the greater number of the gases.

If the proportion of hydrogen deduced from these results, namely, 13.27 per cent, was erroneous, the error was in estimating the specific gravity of either the hydrogen or the oxygen, or both; for the computation of respective volumes has this remarkable advantage, that, being dependent on a general law, it is incapable of error. Before we entered on new observations on the densities of oxygen and hydrogen, we wished to obtain, by a simple method, the confirmation of our doubts. The decomposition of an oxide by hydrogen appeared to us the most accurate and convenient way; and we adopted the following precautions to render the experiment conclusive:

We first procured perfectly pure hydrogen gas. Distilled zinc is not preferable for this object to common zinc, for they both contain the same impurities, namely, lead, tin, copper, iron, cadmium, and sulphur; but on passing the hydrogen through a tube containing fragments of caustic potash slightly moistened, the gas loses its odour completely, and comes out perfectly pure.

Hydrogen gas, obtained by the action of diluted sulphuric acid on zinc, was purified by sending the current through moistened fragments of caustic potash. It was then dried by being transmitted through muriate of lime; after which it was placed in contact with oxide of copper dried and inclosed in a tube, which was united to the apparatus by two tubes of elastic gum, which allowed us to weigh it accurately both before and after the experiment. When the hydrogen had passed in sufficient quantity to expel the atmospheric air, the oxide of copper was heated by a spirit lamp. In the first experiments, the greater part of the newly generated water was received in a liquid state in a small recipient attached to the above-mentioned tube, in order to allow us to examine its purity: in the subsequent ones, the aqueous vapour and the excess of the hydrogen passed through a long column of fused muriate of lime. It is easy to see of how much precision this mode of performing the experiment is capable. Hence the results obtained in the several trials differ but little from each other; and as we were not able to detect any impurity in the water produced, we may consider the following numbers expressing as exactly as possible the composition of this fluid.

From the mean of three experiments it appears that 100 parts by weight of oxygen unite with 12.488 of hydrogen to produce water; which is equivalent to 88.9 per cent. of oxygen, with 11.1 of hydrogen. Whereas the number formerly assumed as the proportion of hydrogen to 100 of oxygen, is 13.27 instead of 12.488, which makes a difference of nearly a twelfth part. We can therefore no longer doubt the reality of the error which we had suspected; but it became necessary to examine the cause of it, by taking anew the densities of oxygen and hydrogen, which we performed in the usual methods, adopting, however, the following precautions, which appear to us of so much importance as to deserve a particular notice.

It has been proved by Mr. Dalton, that no gas insoluble in water can remain confined in contact with this liquid, even for a short time, without absorbing a certain quantity of the gaseous mixture which water always holds in solution. When the density of the confined gas does not materially differ from that of atmospheric air, the addition of the gas which is absorbed from the water produces no material error; but where the confined gas is hydrogen, in particular, it is obvious that an alloy of no more than a hundredth part will produce a prodigious error in the estimated specific gravity. It is very probable that to this cause (which was not known to Messrs. Biot and Arrago) we must attribute the error that affects the number which they have given for the density of hydrogen. We have avoided it by covering the surface of the water that confines it with a stratum of fixed oil, which, as it is well known, makes the passage of the gas from the water much more difficult. We have operated and given the results of our experiments both on dried gas, and on gas saturated with moisture. One may employ either of them indifferently, particularly when the external temperature is not very high. However, it has appeared to us that the observations made on the gases artificially dried accorded better with each other. Not that there is any uncertainty in the data, on which are founded the corrections that must be made for aqueous vapour:

pour; but that, in passing the humid gases from the jar to the balloon glass, it is difficult entirely to avoid the condensation of a minute portion of aqueous vapour, when the sudden expansion of the transferred gas causes a reduction in its temperature.

M. Biot, to avoid long calculations and corrections, often uncertain, has proposed to weigh the exhausted balloon both before and after the weighing of the gas, and to take the mean of these two determinations as the true weight of the balloon at the moment in which it is weighed full of the elastic fluid. For this proceeding to be accurate, the atmospheric changes must go on uniformly, and the first and third weighings should be made at a distance of time nearly equal to the intermediate weighing. For short intervals, this method is not exposed to the risk of any important error; but when elastic fluids obtained by long and difficult processes are operated upon, a considerable time may elapse between the first and the second weighing, and during the double of this interval, the uniformity of the variations may no longer have taken place. We therefore preferred taking the weight of the exhausted balloon immediately after each weighing of it when full of the required gas. A few minutes are sufficient to make the vacuum, and during this short interval, it is very rare that any change in the circumstances of the atmosphere can occur.

The following observations relate to oxygen, hydrogen, azote, and carbonic acid: The oxygen was extracted from chlorate of potash, and was passed through a strong solution of caustic potash, to extract any portion of carbonic acid with which it might be contaminated. The method of obtaining hydrogen has been already described. The carbonic acid disengaged from white marble by means of nitric acid, was made to traverse a long column of powdered crystals of subcarbonate of soda before it reached the vessel that was to receive it. Lastly, the azote was obtained by decomposing ammonia by chlorine, and passing the gas through an acid and an alkaline solution alternately.

The following are the results of the specific gravities of the gases according to our experiments, the gases being perfectly dry, and atmospheric air being = 1.000.

Oxygen	1.1026
Hydrogen	0.0688
Carbonic acid	1.524
Azote	0.976

The gravities of the same gases, as determined by Messrs. Biot and Arrago, are as follows:

Oxygen

Oxygen	1.10359
Hydrogen	0.07321
Carbonic acid	1.519
Azote	0.969

If we take the above proportions in weight of the elements of water, and take the density of oxygen as obtained by our experiments at 1.1026, the specific gravity of the hydrogen will turn out to be 0.0688, but by direct experiment it gave us 0.0687.

It appears, then, that the greatest difference between our results and those of Messrs. Biot and Arrago relates to the density of hydrogen; which confirms what we have said above on the cause of this difference. The increase in the number which we have given for carbonic acid, though small, is sufficient, however, to influence in a sensible manner the number expressing the density of the vapour of carbon on account of its levity; and it appears to us to accord better with the results of the analysis of vegetable substances. Lastly, the density of azote, calculated from our observations, approaches more nearly to that which is deduced from the composition of the nitrates.

For the convenience of those who engage in analytical researches, we have collected in the following table the densities and proportions by weight of several compounds calculated from the bases above given. These numbers should be preferred to those that are obtained by direct analysis, which hardly ever bear the same degree of approximation that may be obtained by inference from the above-mentioned data.—Before we conclude, we may observe that our new results differ but little from those which are given in an anonymous memoir, inserted in the Annals of Philosophy for November 1815 and February 1816; but the English author has added no observations, and the hypotheses which have served him to correct the established numbers being absolutely gratuitous or false, no confidence can be placed in his results.

Table

Table of the Density and Composition of various Bodies.

Name of the Substance.	Specific grav. Atm. air = 1.	Weight of the atom. Oxyg. = 100.	Proportions in weight for 100 parts.		
Oxygen	1.1026	100.			
Hydrogen	0.0688	6.244			
Azote	0.976	88.518			
Vapour of carbon	0.4214	38.218			
Carbonic acid	1.524	138.218	Oxygen 72.35	Carb. 27.65	
Oxide of carbon	0.9727	88.218	Ditto 56.68	Ditto 43.32	
Olefiant gas	0.9804	88.924	Hydr. 14.035	Ditto 85.965	
Carburetted hydrogen gas from marshes	0.5590	50.70[illegible]	Ditto 24.615	Ditto 75.385	
Vapour of water	0.620	56.244	Oxygen 88.9	Hydr. 11.1	
Oxide of azote	1.5273	138.518	Ditto 36.097	Azote 63.903	
Nitrous gas	1.001	94.259	Ditto 55.069	Ditto 44.931	
Hypernitric acid	———	477.036	Ditto 62.888	Ditto 37.112	
Nitrous acid	3.1812	298.518	Ditto 69.320	Ditto 30.680	
Dry nitric acid	———	677.036	Ditto 73.842	Ditto 26.158	
Concentrated nitric acid	———	902.012	Dry acid 75.059	Water 24.941	
Ammonia	0.5912	53.884	Hydr. 17.287	Azote 82.713	
Sub-carbonate of ammonia	———	122.993	Carb. acid 56.190	Amm. 43.810	
Cyanogen	1.8188	164.954	Carbon 45.339	Azote 53.661	
Hydro-cyanic acid	0.9438	85.597	Ditto 44.65	Hydr. 3.645	Az.: 51.705
Vapour of alcohol	1.6004	———	Carbon 52.661	Hydr. 12.896	Oxyg. 34.443
Vapour of ether	2.5808	———	Carbon 65.313	Hydr. 13.329	Oxyg. 21.358

XLIX. *Notices respecting New Books.*

Force Navale de la Grande Bretagne. 2 Vols. 4to. with large Plates, forming the Second Part of Mr. Dupin's *Voyages dans la Grande Bretagne.*

Mr. Dupin, a distinguished member of the Royal Institute of France, and one of the most eminent engineers formed by the Polytechnic School, has conceived the project of examining all our public works and institutions, and of giving to the public what he finds in them worthy of general attention. His object is to present a complete picture of every thing connected with or conducive to the public force, opulence and glory of Great Britain.

To methodise this immense plan, he has divided his subject into four principal parts, military, naval, commercial, and of industry. In the course of last year, he gave to the world the military part, and he has now published the naval one. The author has consequently finished all that relates to the arts of war; the *destruction part* of his work is complete, and what remains to be done relates to the far more valuable part of *production*. If Mr. Dupin shows himself equally accurate, judicious and philosophical, in treating of the last, as he has been in treating of the former, he will raise a monument worthy of our national greatness; and acquire for himself a claim to the gratitude and remembrance both of his own country and of ours.

In a former number of our Magazine, we gave an account of the volume on *military force*; we shall now proceed with giving the account of that on the *naval.*

The judgement which we have given of the first part has been adopted and confirmed by most of the critics who have since reviewed it; and we dare say they will equally join us, in paying to the author a still greater and not less merited tribute of esteem for the two subsequent volumes. In these, not only the institutions upon which the English navy is founded, but all the establishments and public works relating to it, are described and judged not only with an acute, penetrating, and skilful eye, but with an unprejudiced, candid and philosophical mind.

The first volume of the naval part (the third of the collection) presents the *Constitution* of the navy. Under this title the author treats at first of the royal and legislative authorities considered as far as regards their influence and authority upon the navy. Afterwards he considers the nature and extent of the authority of the Admiralty, and of the military officers from the admirals down to the petty officers; he describes the composition and organization of the crews of ships of war and of the royal marines. Then follows the civil part of the naval administration as divided into the departments of the pay-master of the navy, navy board, and surveyors of the navy.

Mr. Dupin examines at great length the office of transports as it was in time of war, and the victualling office. He describes with peculiar minuteness the humane provisions which have been made to secure the health of the troops when embarked and very often crowded in great numbers; and shows from some striking facts the happy result which has been produced by the improvements thus introduced.

Mr. Dupin is still more particular about all the means of improving the food of seamen. He details at length the allowances in food and drink; and expatiates on the care which is taken to procure for our gallant tars the best bread, beer, spirits, meat and fish, as well as the excellent rules for distributing equally and impartially the allowance of a ship's company. Mr. Dupin also speaks in warm terms of the peculiar attentions taken to prevent disease, and gives several remarkable instances of the consequent diminution in the number both of sick and dead on board the ships of the English navy. The author confesses that in addition to his own observations, he has drawn a good deal from the learned productions of Sir Gilbert Blane, formerly one of the principal medical officers in the British navy.

Mr. Dupin has consecrated a chapter of his first volume to the treatment of prisoners of war. He mentions only what he saw of the hulks in which these prisoners were confined, what our illustrious and humane Howard declared to have seen when he visited these places of seclusion, and what is officially avowed in the reports of naval revision. We meet in Mr. Dupin with no such disgusting and injurious declamations as those of General Pillet and other libellous writers However, we must confess that his picture is sad enough to make us desirous to see in future wars, the unhappy prisoner treated in a manner more worthy of the justly boasted British humanity.

Having finished all that concerns the general or as he calls it central administration of the civil affairs of the navy, the author describes the civil administration of the sea ports and dock yards. Here, as well as in most parts of our institutions, the author finds a great superiority on our side compared with the institutions of his own country. So candid a confession from such a judge as Mr. Dupin gives us the best proof that our splendid naval victories are equally due to the unparalleled bravery of our officers and sailors, and to the excellence of the institutions by which such successes are prepared and facilitated.

After explaining the general regulations on which the constitution of our navy is founded, the author considers the state of the navy itself either in time of peace or war. He shows the progress of our naval force since our glorious Revolution to the present day; he finds the number of large ships four times greater; the number of seamen employed on them 3½ times greater; and he declares that, comparing this with the services rendered by the navy at the two epochs of 1688 and 1808, we must be astonished how it is that with so little an increase in number, both of ships and men, we could produce results of so different an importance and magnitude.

We have read with peculiar attention the chapter in which Mr. Dupin considers the number of dead and sick in our navy in time of war. He shows from the data furnished by Dr. Blane, the happy change which has taken place since the last thirty years in the healthiness of our men of war, in consequence of the better victualling, the greater care given to cleanliness, ventilation, and comforts.

The last chapter enumerates all the funds of relief established by British generosity and gratitude in favour of the superannuated officers, sailors and marines, when they are no longer wanted for the public service, or when their infirmities render them unfit to fight the battles of their country: such is the establishment of Greenwich Hospital.

The second volume of the *Force Navale* contains all that relates to studies and works. It is the professional and not the least valuable part of the book.

Under the title of what Mr. Dupin calls *force morale*, he considers at first the popularity of the British navy; and as an example of the veneration shown by the nation for their illustrious dead, presents us with a very animated description of the honours paid to the remains of Nelson after the victory of Trafalgar.

The discipline of our navy, Mr. Dupin declares to be superior to that of any other state in Europe, and shows in what particulars it is essentially remarkable and excellent.

The navy is, of all the military arts, that which is most indebted to science. Mr. Dupin as a geometer and an *academicien* could not fail to notice how science has contributed to the progress made in navigation and naval architecture. He gives the history of the remunerations given by the British government to the discoverers of the best means of finding the longitude at sea; he mentions also the well merited recompenses bestowed on Sir Robt. Seppings and Mr. Brunel, the latter for his block machinery, the former for his improvements in the structure of our men of war.

The author next examines what the English government has done for giving the necessary instruction to the shipwrights and seamen. His accounts of the school for naval architecture, of the Royal College at Portsmouth, and of the Naval Asylum at Greenwich, are particularly interesting.

Six chapters are consecrated to the exercises, naval tactics and battles. We cannot here give a sufficient idea of so important a subject, and must content ourselves with recommending the work itself to the attention of all officers, particularly a remarkable chapter in which Mr. Dupin explains how it happened in the last was, that the Americans obtained successes so unexpected and so unnatural as they did at first. In treating of these subjects Mr. Dupin holds out the exploits of our naval heroes, Nelson, St. Vincent, Exmouth, Sidney Smith, &c. as examples to all the maritime powers, and never suffers himself to be biassed by any undue partiality to shade the splendour which belongs to our naval history.

A large part of the book is consecrated to naval gunnery, which presents to the author many recent and valuable improvements. General Blomefield's and General Congreve's short guns, General Congreve's mounting of naval ordnance, Col. Howard Douglas's double flinted lock, Col. Millar's new chambers for carronades; are the most preeminent improvements or inventions which he describes at length. He gives also a great many ballistic experiments; he sets particular value on those carried on at Woolwich with the ballistic pendulum, under the direction of Dr. Gregory, professor of mathematics at the Royal Military Academy.

Having treated at length of the naval ordnance, he then examines our men of war furnished with all their guns, and means either of attack or defence. He compares the strength of our *wooden walls* with that of the foreign men of war, and almost in every particular relating to military power, acknowledges the decided superiority of our navy.

In treating upon this plan the offensive and defensive strength of our navy, compared with that of the various maritime nations, Mr. Dupin shows himself an able engineer, deep in theoretical knowledge, and possessed of a remarkable power of observation, joined with an uncommon degree of rectitude of mind.

Next to the actual strength of ships the most important object is to give them duration. We have of late greatly improved in this respect. Mr. Dupin bestows on the book recently published on this subject by Mr. Knowles, its due portion of commendation.

To finish his work, Mr. Dupin has devoted twelve chapters to the description of all our dock yards and their dependencies. These magnificent establishments, where have been built, fitted and repaired, the fleets which conquered all the seas of the world, required a no less extensive description. Among the objects which here receive the author's special and favourable notice, are the new smithery erected under the inspection and upon the plans of Mr. Hall; Mr. Hookey's means of binding timber; the saw-mills erected by Mr. Brunel; the extensive and beautiful hydraulic works carried on at Sheerness by Mr. Thomas under the direction and according to the plans of Mr. Rennie, &c. At Portsmouth as well as Chatham Mr. Dupin describes the buildings, docks and machinery, for which we are indebted to Gen. Bentham, formerly superintendent of our naval works.

Mr. Dupin gives also very accurate descriptions of the great naval hospitals, of Haslar at Portsmouth and of Plymouth: with respect to the latter, he acknowledges himself indebted for many valuable informations about the treatment of sick seamen to Dr. Hammick, one of the principal medical officers of our navy.

At Plymouth the most interesting work described by our author is the break-water conducted by Mr. Widby under the inspection and upon the plans of Mr. Rennie. This description, already published in French, translated into English, and received with a merited favour by the public, will be found considerably improved in this volume, to the end of which we are at last arrived.

The naval part is accompanied by a dozen large drawings beautifully engraved, and representing with the greatest nicety the architectural or naval works and machines described in the two volumes.

Such is the great variety of interesting information collected by Mr. Dupin, that we have only been able in this account to indicate the principal heads of the most interesting objects comprised in his descriptions.

No doubt persons well conversant with the subject will find some inaccuracies or mistakes in various parts of the two volumes which we have reviewed; but we can say with the learned critic of Mr. Dupin's military part in the Quarterly Review*, far from being astonished by such mistakes, or inadvertencies, we must only wonder how they are so few in number. We suspect that but few officers either of our army or navy have acquired so extensive, deep and accurate knowledge of our military institutions, studies and works, as this intelligent foreigner exhibits.

An Introduction to the Knowledge of Funguses. With Engravings. 8vo. pp. 20.

It was observed by Linnæus in his *Philosophia Botanica*, about 70 years ago, that the order of Fungi, to the disgrace of science, was then a chaos, botanists being ignorant what might be a species, what a variety. The reproach is still in a great degree applicable, though certainly considerable attention has of late years been bestowed on this department of botany. The purpose of the present neat and unassuming tract is generally to bring into notice the whole tribe of Funguses, the more minute as well as the larger kinds, and particularly to recommend to botanists, and such as are fond of drawing flowers, to take accurate figures accompanied with descriptions of such of them as they may happen to meet with. "What renders it peculiarly desirable," says the author, "for all those who are capable of taking good figures, to pay attention to this tribe, is the difficulty, indeed it may be said of some of the species the impossibility, of preserving them like other plants; and also of conveying them to a distance in a fresh state, to be figured. Some sorts, the *Lycoperdons*, the hard *Boletuses*, and some *Sphærias*, may easily be preserved in a dry state for many years, without materially altering their shape. Others, the *Mucors*, are so tender, that great care must be taken not to shake them, in removing them from their places of growth. There are some of an intermediate substance, which may be preserved in Herbariums in a dried state, and be of great service to botanists; several species of *Agaric* may be thus kept. It is those sorts which when gathered cannot be sent to a distance without losing their shape or colour, which require immediate attention: it may be observed in such cases, that, '*delay is dangerous*,' and that '*there is no to-morrow*;' for a few hours, or less, will make so great a difference in their appearance, as to render it impossible for them to be properly figured by any one."

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* See the last Number.

The genera which the author has adopted, are four more in number than what Linnæus arranged all the species of Fungi under, contained in his works; yet he expresses himself aware that it "will most probably not be thought sufficient to comprehend all the known species." Incomplete as the selection of genera may be, however, he trusts that "it will serve the purpose of affording sufficient information to those who may attentively examine it, to be at no great difficulty in most cases in determining what genus any Funguses they find are placed under in other publications."

Having been favoured by the author with the use of his plates for the illustration of this notice, we subjoin a list of the different genera, with their more prominent distinctive characters, referring our readers to the work itself, for a more detailed explanation.

"Genus 1. *Agaricus*. Fig. 1. Plate III.

"Fungus horizontal with gills or lamellæ on the underside. *Linn.*

"2.

"2. *Boletus.* Fig. 2.

"Fungus horizontal, with pores on the underside.

"3. *Hydnum.* Fig. 3.

"Fungus horizontal with spines on the underside. *Linn.*

"4. *Clathrus.* Fig. 4.

"Volva or wrapper coriaceous, body of the Fungus hollow, cellular, pierced; seeds immersed in a glutinous substance. *Phil. Mag.*

"The generic character of this genus," observes the author, "is totally different from that of Linnæus: it has been altered, in order to include the Stinking Morell, and the Red-headed Morell, both which, in several respects, much resemble the *Clathrus cancellatus* (a foreign species), although in outward form, when at maturity, they appear very different. The specific characters of the three species are as follow (first published in Phil. Mag.):

"*Cl. cancellatus,* which may be called Latticed Clathrus. Corpore globoso fenestrato.

"*Cl. pileatus.* Stinking Morell. Corpore cylindrico, pileo favoso.

"*Cl. capitulatus.* Red-headed Morell. Corpore cylindrico, capitulo corrugato.

"5. *Helvella.* Fig. 5.

"Pileus on a stem, smooth on both sides, seeds thrown out from the under surface.—*Withering.*

"6. *Peziza.* Fig. 6.

"Plant concave, seeds on the upper surface only, discharged by jerks.—*Withering.*

"7. *Nidularia.* Fig. 7.

"Fungus leather-like, bell-shaped sitting, capsules large, flat, fixed by pedicles at the bottom of the bell.—*Withering.*

"8. *Clavaria.* Fig. 8.

"Uniform, upright, club-shaped, seeds emitted from every part of its surface.—*Withering.*

"9. *Auricularia.* Fig. 9.

"Flat, membranaceous, fixed by its whole underside, but becoming detached and turning up with age, seeds discharged slowly from what was the upper, but is now, in its state of maturity, the under surface.—*Withering.*

"10. *Sphæria.* Fig. 10.

"Fructifications mostly spherical, opening at the top; whilst young filled with jelly, when old with a blackish powder.—*Withering.*

"11. *Trichia.* Fig. 11.

"In clusters mostly fixed to a membranacous base, capsules globular or oblong, seeds escaping from its whole surface through openings made by the separation of the fibres.—*Withering.*

"12. *Reticularia.* Fig. 12.

"Roundish or oblong, soft and gelatinous when young, when older firm, friable, tearing open indiscriminately, and discovering seeds entangled in capillary fibres, reticulated membranes or leather-like cases.—*Withering.*

"13. *Lycoperdon.* Fig. 13.

"Roundish, fleshy, firm, becoming powdery and opening at the top, seeds fixed to filaments connected with the inner coat of the plant.—*Withering.*

"14. *Mucor.* Fig. 14.

"Fugacious, head like a dew-drop, at first transparent, afterwards opaque; stem either simple or branched."

The author observes in conclusion with great modesty, that the work, which does his zeal for the cultivation of botany much credit, "is not put forth as even *an attempt* at a *complete System of Fungi* (so far as relates to the *Genera*) but that the object of it is *particularly* to recommend to young people and others, whether botanists or not, who are fond of, and in the practice of, drawing flowers, to take accurate figures, accompanied with descriptions, of such Funguses as they may meet with; and, *generally*, to bring into notice a tribe of vegetables, which, notwithstanding the advanced state of knowledge at the present period, may be called Botanical Outcasts."

A familiar Treatise on Disorders of the Stomach and Bowels, bilious and nervous Affections, with an Attempt to correct the most prevalent Errors in Diet, Exercise, &c. being an Exposition of the most approved Means for the Improvement and Preservation of Health; also a Refutation of the Arguments urged by Sir Richard Phillips against the use of Animal Food; containing likewise the Author's Opinion of the most probable consequences of many prevailing Habits in Society, with practical Hints for their Prevention. By George Shipman, Member of the Royal College of Surgeons in London.

Preparing for Publication.

Dr. J. Reade is preparing for publication, A Treatise on Vision, founded on new and interesting experiments.

We understand that Mr. Parkes is preparing for immediate publication, An Answer to the Accusations contained in a Letter addressed to him by Mr. Richard Phillips, and published in the Twenty-second Number of the Journal of Science, Literature, and the Arts.

L. *Proceedings of Learned Societies.*

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

It is with pleasure that we announce the formation of a Medico-Chirurgical Society in Edinburgh. The Society is formed upon the model of the Medico-Chirurgical Society of London, and has in view precisely similar objects. Most of the Medical Professors in the University, and many of the most respectable practitioners in the City, have co-operated in its formation. Dr. Duncan sen. has been elected its first President; its Sittings commence in the approaching Winter Session.

In addition to ordinary and honorary members, provision is made for the admission of corresponding members; and it is hoped that many, in almost every part of the world, and such especially as retain a grateful recollection of the advantages they derived from their *alma mater*, will not be backward in supplying interesting communications.

Communications may be transmitted to the President of the Society, or to either of the Secretaries according to the following addresses:

Dr. W. P. Alison, 44, Heriot Row, Edinburgh;
Dr. Robert Hamilton, 3, Northumberland-street, Edinburgh.

LI. *Intelligence and Miscellaneous Articles.*

STATUE TO THE MEMORY OF THE LATE SIR JOSEPH BANKS.

At a Meeting, under the Patronage of His Majesty, for the purpose of paying a tribute of respect to the Memory of the late Sir Joseph Banks, held (with the permission of the Council) at the House of the Linnean Society, late the Residence of Sir Joseph Banks, in Soho-square, on the 12th of July 1821,

Sir Humphry Davy, Bart. in the Chair,

It was resolved,—That a Subscription be entered into for a whole-length Marble Statue of the late Sir Joseph Banks, to be executed by Mr. Chantrey, and to be placed in the Hall of the British Museum.

That an application be immediately made to the Trustees of the British Museum for permission to place the statue where proposed.

That the following be a Committee for carrying the above Resolutions into execution:

The Earl of Egremont.
The Earl Spencer, President of the Royal Institution.
The Earl of Aberdeen, President of the Antiquarian Society.
The Earl Whitworth.
Sir Everard Home, Bart.
Sir Humphry Davy, Bart. President of the Royal Society.
William Hyde Wollaston, M.D.
William George Maton, M.D.
Sir James Edward Smith, President of the Linnean Society.
Thomas Andrew Knight, Esq. President of the Horticultural Society.
Joseph Sabine, Esq.

That the Subscriptions be paid into the Banking House of Messrs. Drummonds, at Charing Cross.

That the above Resolutions, together with a List of the Subscribers, be published in the London Newspapers, as soon as the reply of the Trustees of the British Museum has been received.

That, in the mean time, Copies of these Resolutions, and a List of the Subscribers, up to the close of this day, be printed, and transmitted to the present Subscribers, and to such other persons as are likely to become Subscribers to the Monument.

That Joseph Sabine, Esq. be requested to direct the execution of this last Resolution, and to receive Letters and Notices of Subscriptions, to be addressed to him at the House of the Linnean Society.

That Sir Everard Home be empowered to draw on Messrs. Drummonds for the incidental expenses incurred in carrying these Resolutions into effect.

That the thanks of this Meeting be given to Sir Everard Home, to Mr. Sabine, and to Dr. Maton, for the trouble they had taken in preparing the business of this day.

The following Reply to the Application made to the Trustees of the British Museum, in compliance with the Resolution of the Subscribers, has been received.

British Museum, July 11, 1821.

Sir,—I have the honour to acquaint you, that the Minute which accompanied your Letter of yesterday, respecting the wish of the Subscribers to Sir Joseph Banks's Statue, to have it placed in the Hall of the British Museum, has been this day laid before a Committee of Trustees; by whom I am directed to state, that they approve of the proposal submitted to them, and will be very glad to receive the Statue of a person, for whose memory, collectively and individually, they entertain so much respect.

I am, sir, your obedient humble servant,
Henry Ellis,
Secretary.

To Joseph Sabine, Esq.

NEW EXPEDITION TO AFRICA.

His Majesty expressed his desire, a short time since, that an expedition should be formed to explore certain parts of Africa which border upon Egypt. The idea was suggested in consequence of the successful researches of M. Belzoni in the latter country; but the object of the present expedition is of a different character from the pursuits of that gentleman, inasmuch as it is the discovery, not of the ponderous monuments of Egyptian art, but of the remains of Greek and Roman edifices, which, it is conjectured, are scattered in different parts of Libya—a country which those celebrated nations visited, and in which they established colonies at several different periods, but which, it is supposed, no Europeans have since explored.

The gentleman who has been chosen by Government, with the approbation of His Majesty, to superintend this expedition, is Mr. Beechey, many years secretary to Mr. Salt, the English Consul to Egypt, and the constant companion of M. Belzoni in his late indefatigable researches. The Lords of the Admiralty have also afforded every assistance in their power to advance the object of this expedition, by fitting out a small vessel with a complement of men, and intrusting the command to Lieutenant Beechey, who was engaged under Captain Parry in the last Northern Expedition, and the officer from whose drawings were executed some of the engravings that embellish the account of that voyage of which the public are in possession. The vessel is intended to sail round the coast, and to wait upon the expedition, which will only proceed so far in the interior as will be consistent with its safety, or allow an easy return to the coast. The expedition will start from Tripoli, to the Bey of which a communication has been dispatched from this Government to request assistance, which will, no doubt, be afforded, as it has formerly been by that Power upon similar occasions.

Libya, the country about to be explored by our adventurous countrymen, is that which in ancient times contained the two countries of Cyrenaica and Marmorica. The former was called Pentapolis, from the five great cities which it contained; one of which was Berenice, or Hesperis, now Bernic, the spot where the celebrated Gardens of the Hesperides are generally supposed to have existed. Not far distant was Barce or Barca, and Ptolemais, now Tolometa. To the east of the extreme northern

point

point of the coast, called Thycus Promontorium, now Cape Rasat, was Apollonia, now Marza-Susa, or Sousah, formerly the port of Cyrene, that city being situated a little inland: it was founded by Battus, who led thither a Lacedæmonian colony from Thera, one of the Cyclades; and the kingdom was afterwards bequeathed to the Romans by the last of the Ptolemies, surnamed Apion, and was formed by that nation into a province with Crete. The expedition will explore the vestiges of it, which are supposed still to remain under the name of Curin: to the east of this stood the fifth city of ancient Cyrenaica, called Darnis, now Derne.

South of Marmorica (before mentioned), which our countrymen will visit, and in the midst of the sands of the Libyan Desert, was a small and beautiful spot, refreshed by streams and luxuriant with verdure, in which stood the Temple, so celebrated in antiquity, of Jupiter Ammon, said to have been founded by Bacchus in gratitude to his father Jupiter, who appeared to him, when perishing with thirst, in the form of a ram, and showed him a fountain. Here was the Fons Solis, whose waters were cold at noon and hot at night. Here also was the celebrated ancient Oracle, so difficult of access through the Libyan Deserts, and which was consulted by Alexander the Great after a memorable and dangerous journey, the token of which transmitted to posterity, is the ram's horn upon the head of that Conqueror on numerous medals.

The Expedition will, in all probability, be engaged three or four years.

DISAPPEARANCE OF A MOUNTAIN.

The *Journal des Debats* says—" An extraordinary event happened in the environs of Aubenas on the 15th of June last. A loud report was heard, during five or six minutes, to the extent of six miles round. The inhabitants knew not the cause; when a very high mountain, called Gerbier de Jone, at the foot of which springs the Loire, disappeared, and presented nothing but a lake. This mountain was high, and it was difficult to reach the top, at the extremity of which there was a fountain. The commotion was so strong, that it produced an earthquake five leagues in circumference."

OBSERVATORY AT ABO.

The Emperor Alexander has erected at Abo in Finland, a magnificent Observatory, the direction of which he has intrusted to the celebrated astronomer Balbeck.

NEW SHETLAND.

" The large islands of South Shetland, which have been discovered, are five in number. One has been named Livingston's

Island—another Robert's. Some of the harbours are very good; vessels in them being land-locked. Of the three first months of the present year, the mildest experienced there was March; but the seals had mostly retired to the water. A solitary spot or two of something like grass were the only marks of vegetation. No field ice was seen, but innumerable islands were floating about. The flesh of the young seals was often eaten, and was not disagreeable. The remains of the seals were generally left on the beach, after the skins were taken off; but, if convenient, probably much oil might be made."—*American Sentinel.*

EARTHQUAKE.

Batavia Journals of the 28th of April give an account of a terrible earthquake which took place, on the 29th of December last, on the south coast of Celebes. It did immense damage, especially at Boelœkomba, where the sea rose several times a prodigious height, and then falling with incredible rapidity, alternately deluged and left the shore, destroying all the plantations from Bontain to Boelœkomba. Many hundred persons have lost their lives. The fort of Boelœkomba was much damaged, that of Bontain less so.

On the 4th of January, this year, there was another shock of an earthquake; but we not learn that it did any damage.

PHÆNOMENON IN THE TIDES.

Friday, the 7th Sept., a singular phænomenon was observed at Arundel, by the ebbing and flowing of the river Arun, five different times in the course of two hours. When the great earthquake at Lisbon took place on the 1st of November 1755, a similar circumstance accurred, and with the same undulation of the waters, although no tremulous motion was felt.

BOTANY.

A curious and beautiful plant, *Cactus hexagonus*, or six-angled Torch Thistle, was in full bloom last month, in the greenhouse at Chapel-house near Bury St. Edmund's: —its corolla began to expand at six o'clock in the evening, and gradually closed at the same hour of the following morning. It is a native of Surinam, and is seldom known to flower in this country; but experience has shown it may be greatly accelerated by a free exposure to the sun and air during hot and dry weather. The present plant is seven feet high, and supposed to be of about thirty years' growth.

In the nursery of Mr. Boughton, at Lower Wick, near Worcester, is a beautiful and rare specimen of the *Yucca gloriosa*, or Superb Adam's Needle, in full flower, the stem of which is nearly nine feet from the earth, and it has between six and seven hundred blossoms on it either open or to open. This plant is a native of North America, and was first brought into England in the year 1596.

A new species of black currant has been cultivated in Cambridgeshire, the fruit of which is so large, that in some instances a single berry weighs 61 grains, and measures in circumference two inches and a half.

There is at present to be seen in the garden of Mr. Miller, at the Abbey, Edinburgh, what is conceived to be a very great curiosity. In the bed of carnations, there is one root, a stalk from which has produced one carnation half red and half a flesh colour; another wholly a flesh colour spotted with red; and the third a dark red.

The Professor of Agriculture and Botany in the University of Modena, strongly recommends a species of Clover that has not hitherto been cultivated in this country, namely, the *Trifolium incarnatum*, or Crimson Clover. He recommends this plant as the earliest of Trefoils; as the most useful for increasing the forage; as requiring only one ploughing and harrowing to cover the seed; as peculiarly calculated for dry soils, even gravels: and as preferring the mountain to the plain. It is so hardy that it may be sown even in autumn, and it stands severe frosts well. If sown in spring, it will yield a good crop that year. Some experiments have been tried with this plant in Berwickshire, which in a great measure justify what has been urged in its favour.

VACCINATION.

The festival in honour of Dr. Jenner, to whom mankind are indebted for the discovery of Vaccination, was lately celebrated at Berlin by a superb banquet. All the faculty in the city were present, together with several functionaries and statesmen. The Counsellor of State, M. Hufeland, presented at the close of the banquet, lists of the children who had been vaccinated in Prussia during the year 1819, and the result was, that upwards of 400,000 children had been inoculated within that period.

VOLCANO IN THE ISLE OF BOURBON.

[Account of a late explosion, by an eye witness.]

On the 27th of February, at 10 o'clock in the morning, the weather being cloudy, a frightful noise was heard like that of a loud clap of thunder, produced by the explosion of a column of fire and smoke from the crater of the volcano. The clearness of the rest of the day prevented a full enjoyment of this brilliant horror; but on the arrival of night a pillar was perceived, formed of masses of fire and inflamed matter, shooting majestically to a prodigious height, and falling with a crash which inspired terror. The brightness which it diffused was such, that over all the extent of this quarter a letter could be read by the light of this prodigy. Towards the middle of the night three rivers of fire were discovered opening a passage near the summit of the mountain, a little below the crater, and taking a direction perpendicular to the high road. On the 9th of March one of them had passed it, leaving a line of lava 6 feet high by 20 broad, and rolled to the sea over an extent of 30 poles, throwing up the water to such a height, that it fell down in the shape of rain.

At the moment of the eruption, a shower, composed of blackish ashes, of gold coloured glass, and sulphurous particles, fell in the vicinity of the volcano. It rained thus for two hours. On the 9th of March we experienced an earthquake, which was of so short a duration, that we could not determine its direction. From the first moment of the eruption to the day on which I write, the volcano has not ceased to burn. On the 1st of this month, it threw out such a quantity of smoke, that the higher parts of the island were covered by it. On the 2d the rain was so abundant, that the arm of the lava reaching to the sea was extinguished, and on the 4th it could be passed without much danger.

An observer, whom I placed in such a manner as to seize the most minute circumstances which the volcano in activity might present, tells me that at this moment the second arm of the lava has reached the high road on a base double the breadth of the former, or 60 poles, and that the third is 200.

Having long resided in Naples and Sicily, I have ascertained that the lava produced by the volcano of Bourbon does not at all resemble that produced by Vesuvius and Ætna: the lava of the two latter volcanos is compact, hard, and not porous: trinkets and snuff boxes are made of it, which take a polish finer than marble. The pavement of Naples is made of square blocks from Mount Vesuvius, and it is so slippery that in time of rain we might skate upon it as on ice. The lava of Bourbon is a species of scoria, of a black colour, and presents the aspect of iron dross.

(Signed) The Mayor of St. Roze, PREYNE DE BALLERGUE.

St. Roze, April 9.

EGYPT.

Extract of a letter from Rome, dated August, 1821:—"A young Englishman, of the name of Waddington, who has lately arrived in this city, has penetrated upwards of 600 leagues above the second cataract, in following the army of the Pacha of Egypt. In the whole of the way he fell in with only a few small Egyptian

tian monuments, in isolated situations, and of no very remote date; but on his arrival at Schayni, where the Pacha encamped, he discovered thirty-five pyramids, of from 50 to 120 feet in height, but in a very ruinous state. He also saw seven or eight temples, of which one (upwards of 300 feet in length) was covered with hieroglyphics. It is probably in the neighbourhood of these ruins that search should be made for Nabatha, and not the Meroe of the ancients. This traveller has copied some very curious Greek inscriptions. He assures us that he has seen nothing in his travels comparable to the monuments of Nubia, and that he considers that province as the cradle of the arts in Egypt."—*Moniteur*.

A letter from Marseilles of the 11th of August contains the following interesting piece of intelligence:—

"M. Tedenat, son of the French Consul at Alexandria, well known by his discoveries in Upper Egypt, is just arrived at Marseilles with a number of curiosities from that celebrated country. He traced the cataracts of the Nile from their very commencement, and visited the famous city of the hundred gates. He carried his researches into the mountain of granite, which is close to the ruins of that city, and which is opposite to the grand temple. He discovered very fine mummies, and manuscripts on papyrus of the finest kind, and in the most perfect preservation. It is supposed no library in the world possesses any of the kind in a better state of preservation. His most abundant harvest in matters of that description was in the mountain of *Gourna*. He had the singular good fortune to discover a large cable, made of the leaves of the palm-tree, which was used for letting down the bodies of wealthy persons into a well, which were afterwards buried in spacious sepulchral chambers hewn into the side of the granite mountain, more than sixty toises in depth.

"The wells seem to be designed for the concealment of tombs in the interior; and at the present time it is necessary to excavate at all hazards, in order to discover them.

"The sepulchres of Gourna present a work of the most exquisite perfection, whether we consider the hieroglyphic paintings, or the bas-reliefs which adorn all the walls in the interior. What must we think of the patience and talents of the Egyptian artists, who went even into the bowels of the earth to execute imperishable works, and of the power of those kings, who, not satisfied with having raised lofty pyramids which have existed for thousands of years, and which astonish us by their magnificence, have excavated a mountain of more than thirty leagues in extent, for the purpose of depositing mummies, and, if we may be allowed the expression, to assert the immortality of the body, in opposition to the immutable laws of nature, which has a constant tendency to destruction.

"M. Tedenat is carrying these treasures of antiquity to Paris, and will speedily return to Egypt. The Academy of Marseilles has admitted him into the number of its correspondents."—*Journal des Debats.*

FURTHER DISCOVERIES IN EGYPT.

Letters have been received from M. Caillaud, who is now travelling through Egypt and the neighbouring countries, by order of the French Government. They are dated from Dongolah, January 14, 1821. Beyond Wadi-Halfa, the seat of the second cataract, he made some discoveries, which extend still further the domain of Egyptian Antiquities. Not far from Dongolah, the capital of Upper-Nubia, about one hundred leagues above the town of Syene, there exists a large Egyptian monument, which will bear a comparison with one of those of the city of Thebes. Its length is more than three hundred feet, and it contains ninety columns upwards of thirty feet high. Every part of the monument is covered with hieroglyphics and bas-reliefs; the majority of the subjects represent the images which continually occur on the edifices of Egypt—oblations, religious objects, the march of prisoners, &c. Besides figures of the Egyptian character, M. Caillaud remarks among the personages, here and there, the physiognomy of the black race, and occasionally that of the Circassian race. The place where these beautiful ruins are situated is called Selib or Theibe. The remains of the monument have been measured, described, and sketched by this traveller. Six other Egyptian ruins, not so considerable, have been discovered on the banks of the Nile, between the second cataract and Dongolah; in neither of them have Greek inscriptions been found, or any thing which denotes the residence of the Greeks or Romans. It is remarkable that these monuments are not in such good preservation as those of Lower Arabia or of Egypt. This is to be attributed to the almost constant rains which fall in this latitude, as well as to the perishable nature of the free-stone with which they are built.

QUESTIONS ADDRESSED TO NATURALISTS.

[From a German Paper]

The analysis of the earth shows, that it consists of the five following kinds:—1. Calcareous earth;—2. Quartz;—3. Clay;—4. Magnesia;—and 5. Vegetable mould*. It is affirmed, that repeated experiments have proved, that the first four, as well *alone* as *intermixed*, are absolutely unfruitful. If this be true, many thousand plants, which now thrive only in vegetable mould, could not grow on our earth some thousand years ago. Must we adopt the opinion, that plants and vegetables have risen gradually? In East Friesland, if earths are dug up on the sea coast, &c. from a depth of ten or twelve feet, *plants then grow, which are not otherwise to be met with in those parts of the country.* Did these plants exist in the ancient world? Have their seeds retained the germinating power for some thousand years? Can this power be retained so long? or whence do these plants come?

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* Kalkerde, Kieselerde, Thonerde, Bittererde, and Dammerde.

ANTIQUE GLASS.

A cabinet has been opened in Naples in the Studi palace for the antique glasses found in Pompeii and Herculaneum. The collection contains a great variety of forms and colours, and proves that the ancients made use of glass as the moderns do, both in decorating their rooms and in instruments of chemistry. The cabinet contains also a number of cinerary urns, for the most part inclosed in vases of lead.

VESTIGES REVIVED.

The mausoleums at Surat belonging to the English, erected about the middle and end of the 17th century, are in the arabesque style. One, to the memory of Governor Oxenden, 1669, must have been built at an enormous expense; the dome rises to the height of 40 feet surmounted with gothic arches, forming an upper story supported by massive pillars, with stair-cases in the angles leading also to a terrace and entablatures; the diameter of the building 25 feet. This is not so magnificent as one built over a Dutch chief who died about the same time; the inner room of this, where the body is deposited, is of an octagon shape, with regular doors and windows; the sides of it ornamented with Scripture inscriptions and the escutcheons of his family, the whole surmounted with a dome supported by elegant pillars, forming a piazza round it; it is of much larger dimensions than the former one: the name is Vander Heft, 1679. These lofty piles accord not with the humility of the Christian religion, and are evidently borrowed from the Mahomedans, who required room in their mausoleums for the performance of their religious rites: that is, for the attendance of Priests, Fakirs, and Devotees, a fund being allotted for their maintenance by the deceased.—*Bombay Gaz. Dec. 27.*

THE CHAMELEON.

To the Editor of the Calcutta Journal.

Sir,—For the information of those who are fond of the study of Natural History, I beg leave to make known a few remarks upon the Chameleon, from ocular demonstration.

It is commonly believed that this curious little animal has the power of changing its colour at pleasure to the same shade as the substance upon which it is placed, and that its tongue is forked. I have kept Chameleons in a cage several months, narrowly watching them, and have placed them upon different substances for the sake of experiment. I never saw an alteration in their colour, but merely a variation in the shade, from a light yellowish green to a very dark olive green. The mottles were always visible, though similarly changed with the shade. The Chameleon's tongue, which is nearly three parts the length of his body, is blunt at the end, and not unlike a common probe. From the end of it exudes a small quantity of matter, thick, clear, and glutinous; this he uses in obtaining his prey, which consists entirely of insects. He will remain sometimes for an hour with his tongue upon the ground, and when a sufficient quantity of insects has settled upon it, they are all drawn in and devoured. I have seen this animal dart at a fly settled upon a small piece of paper; the fly escaped, but the paper was drawn to the mouth by the cohesive liquid just referred to, and which I have several times particularly examined. The Chameleon possesses the quality, generally attributed to him, of a power of long fasting.

I am, sir, yours obediently,

A.

MATURATION OF FRUITS.

M. Berard has been engaged in a course of experiments to determine what chemical changes take place during the maturation, ripening and decay of *fruits* of various kinds, in the *Annales de Chimie:* his general results are stated as follows; viz.

"Fruits act upon atmospherical air in a different manner to leaves. The former at all times, both in light and darkness, part with carbon to the oxygen of the atmosphere, to produce carbonic acid, and this loss of carbon is essential to ripening, since the process stops if the fruit is immersed in an atmosphere deprived of oxygen, and the fruit itself shrivels and dies. This occurs equally to those fruits which when gathered green are able to ripen of themselves, though separated from their parent tree; but in these the ripening process may be by this means delayed for a certain time, and be completed on restoring them to an oxygenized atmosphere. In this manner peaches, plums, apples, pears, &c. may be preserved unspoilt for from three to ten or twelve

weeks

weeks, inclosed in an air-tight jar, with a quantity of lime and sulphate of iron worked up into a paste with water, which has the property of abstracting oxygen from the air that is in contact with it. The passing from ripeness to decay in fruits is also characterized by the production and evolution of much carbonic acid, and equally requires the presence of an oxygenized medium. The internal changes produced in fruits by the ripening process are particularly distinguished by the production of sugar, which hardly exists in any notable quantity in immature fruits; and it appears to be produced at the expense of part of the gum, and especially of the ligneous fibre.

"Lastly, the change which the woody fibre experiences during maturation continues during the decay of the fruit. It becomes brown; much carbonic acid is given out, and part of the sugar again disappears."

PATENT FIRE SHIELD.

A Mr. Ralph Buckley, of New York, has invented and obtained a patent for a Fire Shield, of which the National Advocate gives the following account:—

"It appears to us the most effectual protection of property from fire ever invented. This shield is intended to protect firemen whilst employed in extinguishing fires, but it is particularly designed to prevent fire from spreading. It is well known that, when a house is on fire, if it even can be saved after the time is lost in bringing up engines, it must necessarily be very much damaged. The evil to be apprehended is the spreading of this devouring element, which frequently lays whole blocks of buildings, and sometimes whole cities, in ashes. This invention is intended to arrest the evil on the spot where it originates, by enabling firemen to approach so near the flames as to protect surrounding property. As this invention is of deep interest to our citizens, and particularly in the southern cities, so much afflicted by fires of late, we deem it necessary to be particular in our explanations. The fire shield is made of a metallic substance; thin, light, and impervious to heat; it is of a length and breadth sufficient to cover the whole person, and it may be used in several different positions. For example: when used in the street, it is firmly fixed on a small platform, with wheels, and a short elevation from the ground. The fireman takes his stand on this platform, and behind the shield; he is dragged by ropes near the current of heat and flames, without being scorched or feeling any inconvenience, and with the hose pipe, or leader in his hand, he directs the water to the part where it is most required. In this way a line of shields may be formed in close order, in front of a powerful heat, and behind which the firemen may stand with safety and play upon the houses with their water pipes. The utility, therefore, of this invention may be seen at a glance; it enables firemen to brave the flames with impunity, whereas, in most instances of excessive heat, they are driven off, and the flames are permitted to spread. The shield is used in another and equally beneficial way. By varying the form, it is carried up stairs to the third story of houses not on fire, but the roof of which requires water, and by a simple machinery carried in hand, it is projected from a window like a painter's platform; the leader is then carried through the house, up stairs, and so out of the window, and is directed by the fireman behind the shield to that part of the adjoining houses which it may be necessary to protect. It is extremely useful in churches, and from steeples, and may be applied in a variety of ways. Firemen have been frequently injured in health and person, by approaching too near the flames, and giving full scope to that intrepidity of character and humanity for which they are distinguished. By this fire shield they will be effectually protected, and it will be found in narrow streets to be peculiarly useful."

PICTURE WRITING.

Among the additions recently made to Dr. Mitchill of America's museum of curiosities is a letter from the Chippewa tribe of Indians to the Sioux, with the answer of the Sioux to the Chippewas, done during the summer of 1820. Both are executed with the point of a knife, or some other hard body, upon the bark of the birch tree. They are examples of *picture writing*, bordering upon the symbolic or hieroglyphic, and show the manner in which the aborigines of North America communicate their ideas at the present day. After having served the purpose for which they were intended, they were procured by Captain Douglas from the banks of the Mississippi, where they had been placed by their authors, and brought home by that gentleman as specimens of the way pursued by those people to transact their public business.

THE WILD ASS.

[From Sir R. Ker Porter's Travels in Persia.]

" The sun was just rising over the summits of the eastern mountains, when my greyhound, Cooley, suddenly darted off in pursuit of an animal which my Persians said, from the glimpse they had of it, was an antelope. I instantly put spurs to my horse, and, followed by Sedak Beg and Mehmander, followed the chase. After an unrelaxed gallop of full three miles, we came up with the dog, who was then within a short stretch of the creature he pursued; and to my surprise, and at first vexation, I saw it to be an ass. But, on a moment's reflection, judging from its fleetness it must be a wild one, a species little known in Europe, but which the Persians prize above all other animals as an object of chase, I determined to approach as near to it as the very swift Arab I was on would carry me. But the single instant of checking my horse to consider had given our game such a head of us, that, notwithstanding all our speed, we could not recover our ground on him. I, however, happened to be considerably before my companions, when, at a certain distance, the animal in its turn made a pause, and allowed me to approach within pistol-shot of him. He then darted off again with the quickness of thought; capering, kicking, and sporting in his flight, as if he were not blown in the least, and the chase were his pastime.

"He appeared to me to be about ten or twelve hands high; the skin smooth, like a deer's, and of a reddish colour; the belly and hinder parts partaking of a silvery grey; his neck was finer than that of a common ass, being longer, and bending like a stag's, and his legs beautifully slender; the head and ears seemed large in proportion to the gracefulness of these forms, and by them I first recognised that the object of my chase was of the ass tribe. The mane was short and black, as was also a tuft which terminated his tail. No line whatever ran along his back or crossed his shoulders, as are seen on the tame species with us. When my followers of the country came up, they regretted I had not shot the creature when he was so within my aim, telling me his flesh is one of the greatest delicacies in Persia: but it would not have been to eat him that I should have been glad to have had him in my possession. The prodigious swiftness and peculiar manner with which he fled across the plain, coincided exactly with the description that Xenophon gives of the same animal in Arabia (vide Anabasis, b. i.). But, above all, it reminded me of the striking portrait drawn by the author of the book of Job. I shall venture to repeat it, since the words will give life and action to the sketch that is to accompany these pages.

"'Who hath loosed the bonds of the wild ass? whose house I have made the wilderness, and the barren land his dwellings! He scorneth the multitude of the city, neither regardeth he the crying of the driver. The range of the mountain is his pasture.'

"I was informed by the Mehmander, who had been in the Desert when making a pilgrimage to the shrine of Ali, that the wild ass of Irak Arabi differs in nothing from the one I had just seen. He had observed them often, for a short time, in the possession of the Arabs, who told him the creature was perfectly untameable. A few days after this discussion. we saw another of these animals, and, pursuing it determinately, had the good fortune, after a hard chase, to kill it and bring it to our quarters. From it I completed my sketch. The Honourable Mountstuart Elphinstone, in his most admirable account of the kingdom of

Cabul, mentions this highly picturesque creature, under the name of Goorkhur; describing it as an inhabitant of the desert between India and Afghanistan, or Caubul. It is called gour by the Persians, and is usually seen in herds, though often single, straying away, as the one I first saw, in the wantonness of liberty. By the national passion for hunting so wild an object, Persia lost one of its most estimable Monarchs, Bahram, surnamed Gour from his fondness for the sport, and general success in the pursuit of an animal almost as fleet as the wind. The scene of this chase was a fine open vale, near to Shiraz, but which had the inconvenience of being intersected by a variety of springs, forming themselves into exceedingly deep ponds, cavemed at the bottom, by nature, to an extent under ground not to be traced. While the King was in the heat of pursuit, his horse came suddenly to the brink of one of these pieces of water, and, tumbling headlong, both horse and rider disappeared. The pond was immediately explored to the utmost of their ability in those days, but the body of the King could not be found. Hence it is supposed that it must have been driven by the stream into one of the subterraneous channels, and there found a watery grave. This event happened fourteen hundred years ago, and yet it forms an interesting tale in the memories of the natives about, to relate to the traveller passing that way."

THE PELICAN.

A pelican was killed about the middle of last month, in Washington, Augusta county, (Alabama,) at a distance of 250 miles from the sea, which measures nine feet from the extremity of one wing to that of the other, six feet from the end of the tail to the point of the bill, which is 14 inches long; and the pouch, or bag connected with the under part of it, is large enough to contain three or four gallons. The body is shaped much like that of a goose, but a little more elongated towards the neck, and being thickly covered with feathers, appears to be about three times as large, though, from its apparent famished state, and the extreme thinness of its bones, the whole bird weighed but 18 pounds. Its tail is shorter than that of of a goose, and its plumage white, except the extremities of the wings from the last joints, outward, which are black. The skin of the bird is preserved.

WAR POISON OF THE INDIANS.

[From Humboldt's Personal Narrative.]

Esmeralda is the most celebrated spot on the Oroonoko for the fabrication of that active poison which is employed in war, in the chase, and, what is singular enough, as a remedy for gastric obstructions. The poison of the ficunas of the Amazon, the upas-tieute of Java, and the *curare* of Guyana, are the most deleterious substances that are known. Raleigh, toward the end of the sixteenth century, had heard the name of *curare* pronounced as being a vegetable substance, with which arrows were envenomed; yet no fixed notions of this poison had reached Europe. The missionaries Gumilla and Gili had not been able to penetrate into the country where the curare is manufactured. Gumilla asserts that this preparable was enveloped in great mystery; that its principal ingredient was furnished by a subterraneous plant, by a tuberose root, which never puts forth leaves, and which is called the root by way of eminence, *raiz de si misma*; that the venomous exhalations, which arise from the pots, cause the old women (the most *useless*) to perish who are chosen to watch over this operation; finally, that these vegetable juices never appear sufficiently concentrated, till a few drops produce *at a distance* a repulsive action on the blood. An Indian wounds himself slightly; and a dart dipped in the liquid *curare* is held near the wound. If it make the blood return to the vessels without having been brought into contact with them, the poison is judged to be sufficiently concentrated. I shall not stop to refute these popular tales collected by Father Gumilla.

When we arrived at Esmeralda, the greater part of the Indians were returning from an excursion which they had made to the east beyond the Rio Padamo, to gather *juvias*, or the fruit of the Bertholletia, and the liana which yields the curare. Their return was celebrated by a festival, which is called in the Mission *la fiesta de las juvias*, and which resembles our harvest homes and vintage feasts. The women had prepared a quantity of fermented liquor, and during two days the Indians were in a state of intoxication. Among nations that attach great importance to the fruits of the palm-trees and of some others useful for the nourishment of man, the period when these fruits are gathered is marked by public rejoicings, and time is divided according to these festivals, which succeed one another in a course invariably the same. We were fortunate enough to find an old Indian less drunk than the rest, who was employed in preparing the *curare* poison from freshly gathered plants. He was the chemist of the place. We found at his dwelling large earthen pots for boiling vegetable juice, shallower vessels to favour the evaporation by a larger surface, and leaves of the plane-tree rolled up in the shape of our filters, and used to filtrate the liquors more or less loaded with fibrous matter. The greatest order and neatness prevailed in this hut, which was transformed into a chemical laboratory. The Indian who was to instruct us, is known throughout the mission by the name of the *master of poison* (*amo del curare*): he had that self-sufficient air and tone of pedantry, of which the pharmacopolists of Europe were formerly accused: "I know," said he, "that the whites have the secret of fabricating soap, and that black powder which has the effect of making a noise and killing animals, when they are wanted. The *curare*, which we prepare from father to son, is superior to any thing you can make *down yonder* (beyond sea). It is the juice of an herb which *kills silently* (without any one knowing whence the stroke comes)."

This chemical operation, to which the *master of the curare* attached so much importance, appears to us extremely simple. The liana (*bujuco*), which is used at Esmeralda for the preparation of the poison, bears the same name as in the forest of Javita. It is the *bejuco de mavacure*, which is gathered in abundance east of the Mission, on the left bank of the Oroonoko, beyond the Rio Amaguaca, in the mountains and granatic lands of Guanaya and Yumariquin.

The juice of the liana, when it has been recently gathered, is not regarded as poisonous; perhaps it acts in a sensible manner only when it is strongly concentrated. It is the bark, and a part of the alburnum, which contains this terrible poison. Branches of the *mavacure* four or five lines in diameter, are scraped with a knife; and the bark that comes off is bruised, and reduced into very thin filaments, on the stone employed for grinding cassava. The venomous juice being yellow, the whole fibrous mass takes this colour. It is thrown into a funnel nine inches high, with an opening four inches wide. This funnel was, of all the instruments of the Indian laboratory, that of which the *master of poison* seemed to be most proud. He asked us repeatedly, if *por alla* (*down yonder*, that is in Europe) we had ever seen any thing to be compared to his *empudo*. It was a leaf of a plantain tree rolled up in the form of a cone, and placed in another stronger cone made of the leaves of the palm-tree. The whole of this apparatus was supported by slight frame work made of the petioli and ribs of palm leaves. A cold infusion is first prepared by pouring water on the fibrous matter, which is the ground bark of the *mavacure*. A yellowish water filters during several hours, drop by drop, through the leafy funnel. This filtered water is the venomous liquor, but it acquires strength only when it is concentrated by evaporation, like molasses in a large earthen pot. The Indian from time to time invited us to taste the liquid; its taste, more or less bitter, decides when the concentration by fire has been carried sufficiently far. There is no danger in this operation, the *curare* being deleterious only when it comes into immediate contact with the blood. The vapours, therefore, that are disengaged from the pans, are not hurtful, notwithstanding what has been asserted on this point by the Missionaries of the Oroonoko. Fontana, in his fine experiments on the poison of the *ticunas* of the river of Amazons, long ago proved, that the vapours arising from this poison when thrown on burning charcoal, may be inhaled without apprehension; and that it is false, as M. de la Condamine has announced, that Indian women, when condemned to death, have been killed by the vapours of the poison of the *ticunas*.

The juice is thickened with a glutinous substance to cause it to stick to the darts, which it renders mortal; but taken internally, the Indians consider the *curare* to be an excellent stomachic. Scarcely a fowl is eaten (adds our author) on the banks of the Oroonoko, which has not been killed with a poisoned arrow. The Missionaries pretend, that the flesh of animals is never so good as when these means are employed. Father Zea, who accompanied us, though ill of a tertian fever, caused every morning the live fowl allotted for our repast to be brought to his hammock, together with an arrow. Notwithstanding his habitual state of weakness, he would not confide this operation, to which he attached great importance, to any other person. Large birds, a guan (*pava de monte*) for instance, or a curassoa (*alector*,) when wounded in the thigh, perish in two or three minutes; but it is often ten or twelve before a pig or a pecari expires*.

BLUE SUN.

To Dr. Tilloch.

SIR,—As I was passing along the Curtain Road, in the parish of Shoreditch, on Saturday the 18th of August last, between 9 and 10 o'clock in the morning, I observed several people looking up as if at something unusual, and on inquiring the cause, I was told the sun appeared blue. I soon saw, to my surprise, the disc of the sun of an *azure* or sky-blue colour. I am not certain that at any one time I saw the whole of the disc of this colour, owing to the clouds which were passing rapidly before it, covering a portion, but I have no doubt that the whole was seen of this colour by others. There can be no question, I think, but that this extraordinary phænomenon was occasioned by some peculiar refractive power in the thinner clouds which were before the sun at the time. The intervals at which I saw this phænomenon were very short, and all the times together I do not believe were many seconds. Independently of this blue colour, the sun that morning attracted the notice of people by its unusual appearance: it has been described as looking like quicksilver, and like varnished silk, and was mistaken for an air balloon.

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* M. Humboldt does not seem to be acquainted with any certain antidote, if such exists, to this fatal poison. Sugar, garlic, the muriate of soda, &c. are mentioned doubtingly.

I have been induced to send you this account, not having met with any one in your Magazine, and with a wish that some of those persons who saw any extraordinary appearance in the look of the sun that morning, will communicate their observations to you, stating the *time* and *place* where they observed it.

Walthamstow, Essex, 11th September, 1821. B. M. FORSTER.

LITHOGRAPHY.

An experiment has lately been made to take off impressions from plants by lithographic printing, which, although it did not succeed so well as was desirable, leaves little doubt but this method may prove of considerable use to botanists.

A specimen of *Sibthorpia europæa*, which was gathered several years ago in Cornwall, was, we understand, covered with lithographic ink, and impressed on the stone, from which several impressions were taken. There is a well-known method made use of for taking impressions of the leaves of vegetables by covering them with printers'-ink, and then impressing them on paper. The benefit likely to arise from impressing plants on stone, is owing to the facility of multiplying copies much more accurate in some respects than a drawing can be expected to be.

ECONOMICAL PROCESS FOR SEPARATING SILVER AND COPPER.

Dissolve the alloy in nitric acid, and evaporate the solution to dryness, in a glass vessel. Place the salt in an iron spoon over a moderate fire, and keep the mixture in fusion till it entirely ceases to afford bubbles, when it is to be poured out upon an oiled plate. To be certain that all the nitrate of copper is converted into the black oxide of copper, dissolve a small portion of it in water, and test it with ammonia. If the solution, which ought to be at first clear and limpid, does not acquire the slightest shade of blue, it may be concluded that the nitrate of silver obtained is quite free from copper. If not so, the fusion must be continued a few seconds longer. The black product is to be dissolved in cold water; and the solution being filtered, the nitrate of silver passes through in a state of purity. By washing the oxide which remains upon the filter, the small portion of nitrate of silver with which it may be impregnated will be removed: the oxide is then to be dried. The nitrate of silver is afterwards treated differently, according to the use to which it is to be applied.

This process is more simple, expeditious, and accurate, than the common method of separating silver from copper by the humid way.

FISH FLOUR.

"The Indians in all the Upper Oroonoko fry fish, dry them in the sun, and reduce them to powder without separating the bones. I have seen masses of fifty or sixty pounds of this flour, which resembles that of cassava. When it is wanted for eating, it is mixed with water and reduced to a paste. In every climate the abundance of fish has led to the invention of the same means of preserving them. Pliny and Diodorus Siculus have described the fish bread of the Ichthyophagous nations, that dwelt on the Persian Gulf, and the shores of the Red Sea."—*Humboldt.*

LIST OF PATENTS FOR NEW INVENTIONS.

To William Lane, of Birmingham, jack maker, for certain improvements on horizontal roasting jacks, which improvements are applicable to other useful purposes.—Dated 23d August 1821.—2 months allowed to enrol specification.

To David Gordon, of Edinburgh, now residing in Stranraer, esq., for certain improvements in the construction of harness for animals of draught and burthen.—8th Sept.—6 months.

To Bevington Gibbins, of Melin Crythen Works, near Neath, Glamorganshire, chemist, (one of the people called Quakers,) and Charles Hunnings Wilkinson, of Bath, M.D., for an improved retort or vessel for making coal and other gas; and for distillation, evaporation, and concentration of acids and other substances.—8th Sept.—2 months.

To Dominique Pierre Deurbroucq, of King-street, Soho, gent., in consequence of a communication made to him by a certain foreigner residing abroad, for an apparatus for condensing the alcoholic steams arising from spirituous liquors, such as wine, brandy, beer, cyder, and other spirituous liquors, during their fermentation.—11th Sept —6 months.

To Richard Francis Hawkins, of Plumstead, Kent, master mariner, for certain improvements in the construction of anchors.—11th Sept.—6 months.

To William Webster, of George-court, Princes-street, Soho, gun-maker, for certain improvements in the mechanism of and appertaining to Forsyth's rollers, magazine for the discharge of fowling-pieces and fire-arms in general by means of percussion.—14th Sept.—2 months.

To William Losh, of Newcastle-upon-Tyne, iron-founder, for certain improvements in the construction of iron rails for railways.—14th Sept.—2 months.

To James Gladston, of Liverpool, iron-monger, for his method of increasing the strength of timber.—20th Sept.—6 months.

BAROMETRIC OBSERVATIONS.

Pocklington, Yorkshire, Aug. 24, 1821.

SIR,—The following observations I made here on Monday the 13th instant, at the hours given below.

Clock.	Barom.	Thermom. in doors	Thermom. out doors	Wind.	Weather.
8h	29·843	62·6	55·5	S.E. by S.	Calm: clear, except a few white clouds.
9	29·838	63·2	62·0	S.	Gentle breezes: sky covered with thin gray clouds.
10	29·835	64·0	65·5	S.W. by S.	Gentle breezes: clear and cloudy.
11	29·818	64·5	67·0	S.W.	Warm, with gentle breezes: clear and cloudy.
12	29·810	65.4	67·3	S.W.	Warm, with gentle breezes: clear and cloudy. Bright sunshine.

I am, sir, yours truly,

WILLIAM ROGERSON, jun.

Observations made by Dr. WM. BURNEY, at Gosport; the basin of the barometer above low-water mark being 50 feet.

Hour.	Barom.	Ther. att.	Ther. det.	Hygr.	Wind.	State of the Weather.
1821. A.M.						
	Inches.	°	°	°		
Sept. 10. 8h	29·84	63	62	85	W.S.W.	At half past 7 A.M. a bright parhelion appeared to the north of, and 22° 40′ distant from, the sun, and at 8 o'clock a rainbow appeared to the westward, succeeded soon afterwards by an extensive *nimbus*, and a smart shower of rain.
9	29·85	62	60	86	W.S.W.	At a quarter before nine, another rainbow appeared, with bright red, orange, yellow, green, and purple colours, and the shower in which it was situated continued 20 minutes; yet the cirrose crown of the *nimbus* was scarcely perceptible.
10	29·85	63	65	86	W.S.W	*Cumulostrati* passing over with a brisk wind.
11	29.87	66	68	75	W.S.W.	The same modifications as at 10 o'clock, with sunshine and a strong breeze.
12	29 88	68	69	73	W. by S.	Sunshine and a few drops of rain from a passing *cumulostratus* cloud, and very bright semicircular *cumuli* to the northward.
P.M.						
1	29 90	69	68	70	W.	Sunshine, with passing beds of *cirrostratus*, and *cumulostrati* beneath them.

N. B. From some remarks made by Mr. Bevan in the last Number of this work, as objections to my wheel barometer, it is necessary to state that the above observations were made with an upright portable barometer, manufactured in the best manner by the late Mr. George Adams, Fleet-street: that I have sent Mr. Farey the four sets of observations made with this barometer in June, July, August, and September, at the same hours as those published were made, and shall in future use it without making any reductions for temperature.

Leighton, Sept. 22, 1821.

Dear Sir,—I beg leave to send you the observations made at this place on the barometer, &c. on the 10th instant.

1821.	Barom.	Ther. att.	Ther. det	Wind	Denom	Weather
8h	29·461	57½	55	S.W.	small.	Fine.
9	29·474	59	59	S.W.	do.	Cloudy.
10	29·476	60	60	S.W.	do.	Do.
11	29·483	60½	62	S.W.	do.	Do.
12	29·494	60½	59	S.W.	do.	Do.
1	29·500	60	11	W.	do.	Do.

The thermometers suspended near the middle of the barometrical tube averaged 3° more than the attached thermometer in the basin.

The observations made by Colonel Beaufoy at Bushey.

1821.	Barom.	Ther. att.	Ther. det	Wind.	Denom.	Weather.
8h	29·234	57·7	55	S.W. by S.	fresh.	Cloudy.
9	29·242	58·	57	W.S.W.	do.	Do.
10	29·238	58·3	58	S.W.	moder.	Do.
11	29·243	58·7	60	W.S.W.	squally	Rain.
12	29·251	59·7	60	W.	fresh.	Fine.

By calculations of Colonel Beaufoy. from the observations of last month, the height of Bushey appears 225·16 feet above Leighton. I am, dear sir, yours very truly,

B. Bevan.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS			
Aug. 15	17	66·	29·60	Fine
16	18	76·	29·50	Ditto
17	19	71·	29·75	Ditto
18	20	72·	29·55	Ditto
19	21	73·5	29·75	Ditto
20	22	79·	29·70	Ditto
21	23	75·5	29·70	Ditto
22	24	66·	29·75	Ditto
23	25	72·5	29·56	Ditto
24	26	73·	29·50	Ditto
25	27	73·	29·50	Ditto
26	28	61·5	29·65	Cloudy—rain P.M.
27	new	63·5	29·85	Ditto
28	2	58·	29·70	Rain
29	3	62·	29·53	Cloudy—heavy rain P.M.
30	4	66·	29·40	Ditto—heavy rain A.M.—rain at night.
31	5	67·	29·35	Ditto—heavy rain P.M.
Sept. 1	6	64·	29·55	Ditto
2	7	69·	29·70	Fine
3	8	76·	29·58	Ditto
4	9	75·5	29·15	Ditto
5	10	66·5	29·45	Cloudy
6	11	69·	29·45	Rain
7	12	73·5	29·13	Cloudy—heavy rain with thunder and lightning A.M.
8	13	65·	29·27	Ditto—heavy rain A.M.
9	14	67·5	29·27	Fine—heavy rain P.M.
10	15	62·	29·27	Cloudy—rain A.M.
11	full	65·5	29·60	Fine
12	17	65·	29·22	Cloudy—heavy rain A.M.
13	18	62·	29·60	Fine
14	19	64·	29·65	Ditto

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1821.	Thermometer.			Height of the Barom. Inches.	Weather.
	8 o'Clock Morning.	Noon.	11 o'Clock Night.		
Aug. 27	57	64	55	30·18	Cloudy
28	55	60	52	29·96	Cloudy
29	57	57	58	·81	Rain
30	62	72	67	·76	Fair
31	63	70	63	·76	Show. with Thund.
Sept. 1	62	69	64	·97	Showery
2	63	73	63	30·14	Fair
3	62	72	64	·02	Fair
4	64	73	60	29·88	Fair
5	60	72	63	·96	Fair
6	66	73	66	30·04	Showery
7	67	69	64	29·72	Fair
8	60	66	59	·72	Showery
9	60	68	57	·76	Showery
10	58	66	55	·77	Showery
11	55	66	55	30·08	Fair
12	58	69	56	29·68	Stormy
13	56	66	54	30.02	Fair
14	54	68	54	·05	Rain
15	50	67	62	·27	Fair
16	63	72	65	·27	Fair
17	65	71	66	·17	Fair
18	64	70	56	29·98	Fair
19	56	66	55	·95	Fair
20	54	60	60	·97	Rain
21	60	65	61	·75	Showery
22	59	68	60	·80	Fair
23	60	67	58	·72	Foggy
24	56	66	54	·74	Showery
25	52	64	61	30 04	Fair
26	62	69	60	·04	Show. of small rain.

N.B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the
10th Sept. 8 o'Clock M. Barom. 29·736 Ther. attached 59° Detached 58
— — 10 — — — — ·760 — — — 60 — — 66
— — 1 — N. — — ·778 — — — 64 — — 66

LII. *Some Experiments made with a view to the Detection and Prevention of Frauds in the Sale of skimmed Milk; together with an Account of a simple Lactometer for effecting that Purpose. By* EDMUND DAVY, *Esq. Professor of Chemistry and Secretary to the Royal Cork Institution.*

SKIMMED milk, as is well known, is used to a very great extent in Ireland, and especially in the South*, where it forms an indispensable part of the subsistence of the lower orders. Potatoes and skimmed milk, indeed, constitute almost their sole, or at least their principal food. It is therefore of much importance, that an article which essentially contributes to the support of a very large portion of the community, should be supplied in a genuine unadulterated state. Unfortunately this has not been the case. Much of the skimmed milk exposed for sale in our milk markets, has been largely adulterated with water; and for want of the means of detection, this fraud has been practised with impunity, not only in Cork, but also in other parts of the country. An unsuccessful attempt had been made to remedy this evil. Persons called *tasters* were appointed to inspect the milk markets in Cork, and empowered to detain such milk as they supposed to be adulterated. But the inadequacy of such a mode of prevention must be apparent; for nothing can be more vague and uncertain than decisions founded upon the mere taste and appearance of milk. In consequence of the total incompetency of *tasters*, to prevent the commission of frauds in the sale of skimmed milk, a Committee composed of respectable farmers, &c. was formed for the express purpose of putting a stop to this disgraceful practice, so injurious both to the poor, and to the fair dealer. The Committee waited upon our active chief magistrate Sir Anthony Perriere, knt., to apprize him of their intention, and to solicit his assistance. Anxious to co-operate with the Committee in promoting a measure of acknowledged public utility, Sir Anthony afforded every assistance in his power, and promised to adopt any practical remedy which should be devised. At his suggestion, the Committee consulted me as to the best means of carrying their design into execution, and in compliance with their request, I directed my best attention to the consideration of the subject. An instrument on the principle of the hydrometer, seemed to promise the simplest means that could be employed for the detection and prevention of frauds in the sale of skimmed milk; but whether it was practicable to construct such an instrument, depended upon circumstances which could only be determined by experiment. In order to satisfy myself on this point, I commenced a series of experiments on skimmed milk in February last, which have, with occasional interruptions, occupied me ever since. I did not at first indulge any sanguine expectations as to the result of my investigation; but after I had carefully made above a hundred experiments upon *genuine* skimmed milk procured from many of the principal dairy farms, embracing all the varieties of cattle, soil, and modes of feeding, common to this part of the country; and also examined many specimens of *adulterated* skimmed milk from the markets, I have at length ventured to construct a simple lactometer (on the well known principle of the hydrometer), the use of which, I have no doubt, will effectually prevent the frauds now practised in the sale of skimmed milk.

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* The sale of skimmed milk in the markets of Cork alone, I am informed, amounts to about 1000*l.* per week.

Before I describe this instrument, it may be proper briefly to notice the circumstances which led to its construction. My first experiments were directed to ascertain whether any uniformity exists in the density of different specimens of genuine skimmed milk; accordingly I procured this article from many of the principal dairy farms, and from private houses in the neighbourhood. I also obtained new milk from the same sources, which I skimmed myself, after suffering the cream to remain on it about the usual time. The greater number of those specimens were of the specific gravity 1·037 and 1·0375. Some were higher, but the highest was 1·040, and the lowest 1·036, the thermometer being at 50°. These experiments, confirmed by others which I afterwards made, led me to conclude that a considerable degree of uniformity prevails in the density of genuine skimmed milk; and this uniformity, I presume, would be still greater, if due allowance were made for accidental circumstances connected with the experiments, which, though not easy to appreciate, must, to a certain extent, influence the specific gravity of milk; as for example, slight variations of temperature and of the balance employed; to which must be added the unequal exposure to the atmosphere of the several specimens of milk examined. In reference to this last particular, it is proper to state that I found only one specimen of milk of so high a specific gravity as 1·040; and in this instance the cream had been suffered to remain on the milk for above three days, and its specific gravity was not taken until some hours after it had been skimmed. These circumstances incline me to refer its superior density to evaporation, owing to protracted exposure to the atmosphere.

After I was satisfied concerning the degree of uniformity which exists in the density of genuine skimmed milk, my next object was to examine the skimmed milk brought to the markets in Cork, in order to ascertain the nature of the adulterations practised in the sale of this article. Accordingly, I procured at different times, a great number of specimens from the milk markets; and on submitting them to a careful examination, I found that some were genuine, and of course corresponded with good skimmed milk in every particular. Others were adulterated in different degrees, but the only foreign substance I could detect in the adulterated specimens, was water. By adding a certain quantity of water to genuine skimmed milk, it became of the same density as the adulterated milks from the markets. By simple distillation, the adulterated milks furnished pure water, and became of the same density as genuine milk. In some cases, I found skimmed milk from the markets adulterated with more than one-fifth of water; in other instances, with about one-sixth, one-seventh, and one-eighth of water. The worst of the adulterated milks from the markets was of the specific gravity 1·026, the highest of the genuine milks from the markets was 1·039, the thermometer being at 50°.

It is, I believe, a common opinion that skimmed milk is adulterated with other substances besides water; as for example, chalk, flour, starch, sugar, &c. which are said to be used for the purpose of concealing the water, by communicating as circumstances may require a certain degree of whiteness, thickness, or sweetness to milk. I have made a number of experiments to ascertain the correctness of this notion, and I am convinced the opinion is not well founded. Chalk is perfectly insoluble in skimmed milk, and soon subsides when mixed with it, on account of its superior density. Flour and starch increase the density of skimmed milk, but this effect is only temporary; for, not being soluble, they gradually subside. The high price of sugar, were there no other consideration, precludes its use; for I have found by experiment that it would be too expensive even if it could be procured at the low rate of four-pence per pound.

Those experiments on the density of genuine and adulterated skimmed milk (already noticed), which were made at the temperature of 50°, I have since repeated at 60° Fahr. with similar results, making due allowance for the difference of temperature. I have examined the density of a great number of different specimens of genuine skimmed milk, but have not found any of a lower specific gravity than 1·035 at 60° of Fahrenheit.

All my experiments concur to prove, that in the neighbourhood of Cork, genuine skimmed milk obtained under nearly similar circumstances, varies comparatively but little in its density; and that the only substance used to adulterate this article for the markets in Cork, is water.

Skimmed milk and water combine without undergoing any sensible alteration of volume, or condensation. Skimmed milk is of much greater specific gravity than water, and its density is diminished in direct proportion to the quantity of water added to it. On those facts the lactometer I have made depends; it is exclusively adapted to skimmed milk, in which respect, as well as in simplicity of construction, it differs from the ingenious instrument of Mr. Dicas.

Description of the Lactometer, &c.

This instrument (as will appear from the accompanying plate) differs but little in form from the common hydrometer. Its distinction is to be found in its scale, which is adapted to skimmed milk. It is made of brass, and consists of a pear-shaped bulb, at the top of which is a graduated stem, and at the bottom a brass wire to the end of which a weight is screwed. The scale begins about three-fourths of an inch from the bottom of the stem, and is marked 0, which corresponds with the specific gravity of the lightest genuine skimmed milk, or 1·035, distilled water being 1·000. The dots and figures which extend from 0 to 35, indicate "parts of water in 100 parts skimmed milk at 60°," as is engraved on the reverse of the stem, and has been ascertained by experiment. The instrument is constructed for the temperature of 60° of Fahr., a point judged the most convenient, as it agrees very nearly with the temperature of the milk brought to our markets during the summer. As all fluids expand by heat and contract by cold, in using the lactometer an allowance must be made of 1° on the instrument for every 3° of temperature, that the milk under examination is either above or below 60° of Fahr. Thus the lactometer, which would remain at 0 in milk of the temperature of 60°, would sink 1° below 0, if the temperature of the milk were increased to 63°, 2° if it were raised to 66°, &c. And on the contrary, if the temperature of the same milk were reduced to 57°, the instrument would then experience a rise above 0 equal to 1°, &c. This lactometer is made by Mr. Bennett, mathematical instrument maker, Cork, and sold in a tin case, either with or without a small thermometer. It is scarcely necessary to give directions for using so simple an instrument. All that is required is, to fill the tin case with the milk to be examined, immerse the lactometer in the milk, and observe the point at which it remains stationary after it rises. Note also the temperature of the milk, and, if necessary, make the allowance directed for expansion or contraction of volume.

Before this lactometer was used in the milk markets, experiments were made with it in the presence of the Mayor of Cork, Sir A. Perriere, Knt., the Committee of whom I have spoken, and other gentlemen, who expressed themselves satisfied with the accuracy and delicacy of the instrument. The first morning it was employed in the milk markets of Cork, the Mayor, some of the Committee and myself attended, when the Mayor seized thirty-eight churns of skimmed milk, containing above 2000 pottles. The lactometer stood in most of it at 20°; the thermometer being at 58°, which indicated about one-sixth of water. In the evening of the same day, we again visited the very same markets, but found the milk in all of them so much improved that not a single churn was seized. Shortly after, a special public Meeting of the Farmers and Dairymen who supply the markets with milk was summoned, at which the Mayor of Cork presided. In the presence of the Meeting he made several experiments with the lactometer, which were deemed satisfactory. The Mayor then directed the Market Jurors in future to employ the lactometer in the milk markets, and to detain all skimmed milk, in which the instrument sunk 5° below 0, or the point which represents genuine skimmed milk of lowest density. This allowance of 5° was made to avoid being too strict, on the first use of the new instrument. Since the lactometer was first used in the milk markets, the skimmed milk exposed for sale in Cork has been materially improved in quality; and hence comparatively few seizures have been made, though the instrument has now been employed above two months. Dairymen, who have once forfeited their milk, now find they can no longer water it with impunity, and are beginning to relinquish the practice. The same dairy farms, which lately sent milk to the markets in which the lactometer stood at 20°, now furnish milk in which the instrument stands at 0. Besides the evidence already adduced that water is the only substance used to adulterate skimmed milk in this neighbourhood, the fact has been repeatedly admitted by those concerned in the sale of this article. I have been credibly informed that persons have been hired for the purpose of watering milk, and that in this way hundreds and even thousands of pounds have been annually pocketed. This fraud has hitherto been suffered with impunity, merely for want of some simple means of detection. It is every where so easy and practicable, and may be carried to a great extent without being perceptible to the taste or appearance, though it may be readily discovered by means of the lactometer, which is well adapted, not only for markets where skimmed milk is sold, but also for all public establishments where it is used in large quantities.

I think it proper to state, why the scale of the lactometer has not yet been extended above 0, though a vacant space remains on the stem for this purpose, as may been by a reference to fig. 1, (Pl. IV.)

Fig. 1. The lactometer.

Fig. 2. The reverse of the stem.

Fig. 3. The tin case with a small case attached to it for a thermometer.

Fig. 4. The stem of fig. 1, with the scale extended as it should be to 10° above 0.

My design at first was to make the instrument so simple, as to afford the means of detecting water in skimmed milk, at all seasons of the year, without the aid of a thermometer; an object which, with a little explanation, I think it capable of effecting to a sufficient degree of nicety for most practical uses. As the lactometer is constructed for the temperature of 60° of Fahr., and every degree on its scale is equal to 3 degrees of Fahr., an allowance of 2 or 3 degrees below 0 would be amply sufficient for our warmest summer weather. For though the temperature of the air in this season is often much higher than 60°; yet the temperature of the skimmed milk brought to our markets, I have always found to be from 3 to 8 degrees lower than that of the atmosphere, owing to the coolness of dairies, and the effect of evaporation from the milk exposed in large surfaces to the air.

The lactometer, then, in the summer season, should not sink in genuine skimmed milk below 0, except in cases when the weather is very hot, when it might fall to about 2 or 3 degrees below 0. In the autumn and spring, the temperature being much lower than in the summer, the density of the milk will of course be increased, and the instrument should rise in it from about 3 to 6 degrees above 0. In the winter, in like manner, from a further diminution of temperature, there will be a proportionate increase of density in the milk, and the lactometer should rise in it from about 7 degrees to about 10 degrees above 0, or to the bulb. Now, I think, frauds could not be practised to any extent, in the sale of skimmed milk, without detection, if the lactometer, fig. 1, were employed, and a little attention paid to what has been given in explanation of it; yet, as the temperature is very variable in the different seasons, the use of a thermometer is very desirable, especially in cases where the lactometer is employed in markets. The additional expense is only about four shillings, and the trouble would be very little; for the trial of the temperature of a single churn of milk, would furnish indications nearly accurate with regard to all the others. In all cases in which adulterated milk may be seized by the proper authority, the temperature of each churn should be carefully noted down; for too much attention cannot be paid to accuracy, wherever property is liable to be forfeited.

The vacant space above 0, in the lactometer, fig. 1, should be filled up by extending the scale 10 degrees, as is represented in fig. 4, which will adapt the instrument to all temperatures from 30° of Fah. to 60° and upwards; each degree on the scale being equal to the expansion occasioned by 3° of Fah. It is not necessary that the scale of the lactometer should extend to 35° below 0, as in fig. 1. In some of the instruments which have been made for the markets, the scale has only reached to 25°, which seems quite sufficient.

I have found a considerable degree of uniformity in the density of a number of specimens of new milk, which I examined. I have made several experiments in the hope of being able to apply a similar instrument to detect the frauds practised in the sale of new milk; but I fear this is impracticable, because both water and skimmed milk are employed to adulterate new milk; and as the one is lighter and the other heavier than new milk, there would be no difficulty in so proportioning both, as to make the adulterated correspond with genuine new milk in density.

Royal Cork Institution, Sept. 17, 1821.

LIII. *Description of a new Method of forming Crucibles. By Mr.* CHARLES CAMERON, *Glasgow**.

THE Dutch have long enjoyed an almost exclusive monopoly in the manufacture of the small melting-pot, or clay crucible, used by the jeweller and silversmith. The English potter has hitherto failed in imitating those imported from Holland, either in point of shape or quality, in sustaining the sudden transitions of temperature to which they are subjected. In consequence of their superiority, they were an article of great interest to the jeweller during the period of the late war; sometimes they could not be procured, and at other times they sold at five and six times their present price. The English melting-pot was then in request from necessity; it is now entirely out of the market. About two years ago I was led, by a curious train of reasoning, to conceive the practicability of forming crucibles similar to the Dutch, by a simple method, that of moulds made of sulphate of lime or stucco, which would easily give any required form.

I established a small manufactory of them, and carried it on for some time; but owing to particular circumstances, I was obliged to relinquish it, after it had arrived at a state of perfection. Having found it to be the opinion of my friends that the process should not be lost, I have been induced to draw up the following account of it for the Edinburgh Philosophical Journal.

For each of the different sizes of the crucibles, I formed ten or twelve dozen of moulds of stucco, burnt and powdered in the usual manner. For the first mould of each size, I formed a piece of soft pipe clay into the shape of the intended crucible, and laid it with its mouth downwards on a flat surface, and inclosed it with a cylinder of white-iron, distant about half an inch from the angular points of the crucible, and about an inch and a half higher than its bottom; then mixing the stucco with water, poured it into the cylinder. When the stucco was sufficiently set, I removed the white-iron, picked out the clay, and dried the mould; I then squeezed soft clay into the mould, which on standing a few minutes, easily came out again. It was inclosed in the cylinder, and stucco poured round it, which formed a second mould, continuing to do so until I had procured the number wanted. They were then all put into a stove, and completely dried ready for use.

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* From the Edinburgh Philosophical Journal.

In the preparation of the fire-clay for the crucibles, I followed precisely the same process used at the potteries, by mixing it with a very large quantity of water, and putting the whole through a No. 9 silk searce. On allowing the whole to stand a few hours, the clay subsided, and in pouring off the clear water, I procured the clay or slip of the consistence of thick cream. On weighing a gallon of it, I found the proportion of clay it contained, and added sand to the whole, in the proportion of seven of sand to seventeen of clay; I then stirred and mixed the whole completely, when it was ready for use. I next took my moulds, previously dried, and arranged them in parallel rows on a table, and successively filled them with the prepared slip. By the time I had filled four or five dozen, I returned to the one first filled, and began alternately to pour the slip out of them, leaving a small quantity unpoured out, which subsided, and gave the requisite thickness to the bottom. In each of the moulds so filled, a crucible is completely formed by the abstraction of the water of the slip, in contact with, and adjoining to, the porous substance of the stucco mould. The crucible will be either thicker or thinner in proportion to the time the slip has remained in it. Five or six dozen will not require more than fifteen minutes in being formed. The moulds with their contents are then removed to a stove, placed on their side and built one above the other. In a short time, from the contraction of the clay, the crucibles easily part from the moulds, and are removed by introducing the finger into them. The moulds are allowed to remain in their situation until the water they had absorbed is completely evaporated, when they are again ready for refilling, and will last for years. The crucibles remain in the stove until dry, after which they are burned in a kiln in the usual manner.

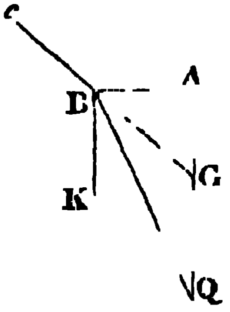
The process is simple, and combines the advantages of forming them with great facility, and giving them the required shape, which cannot be accomplished at once on the potter's wheel. One man and a boy are capable of making from ten to twelve hundred per day. The principle is peculiarly adapted for the formation of a number of chemical apparatus, muffles, retorts, tubes, &c.

LIV. *On Refraction.* By J. Reade, *M.D.*

To Dr. Tilloch.

Sir, — A very common experiment, no less interesting than surprising, is shown in lecture-rooms for the purpose of illustrating the theory of refraction. A piece of money is placed at the bottom of an empty basin, the experimenter retiring until the edge intercepts the object: an assistant then pours in water; the piece of money seems to rise over the edge, becoming perfectly visible and well defined. This experiment seldom fails to surprise the audience, handed down from one generation to another, even from the days of Aristotle; yet I am led to believe the real cause is little understood. Mr. Harris gives the following explanation in his Optics, page 25:

"Hence (says this writer) we have the common phænomenon of a shilling or other object placed in an empty vessel, appearing to be elevated higher and higher as the vessel is filled with water. Suppose the vessel empty, B K its side, and Q the object at the bottom; if the eye be at *c*, the object will be hid by the side B K, but by filling the vessel it will become visible and be seen at G; the ray Q B being refracted or bent into B *c*; and if the eye be so placed as to see the object at Q when the vessel was empty; while it is filling the object will appear to rise gradually in the line Q G. Hence the piece of money appears one quarter nearer the eye than it really is: and on the same principle a river is one quarter deeper than it appears. Q A : G A : : 4 : 3."



Independently of those experiments, there are insurmountable objections to this reasoning. How can any bending of the rays of light bring the object nearer to the eye? If we bend a piece of iron wire, we certainly shorten the length it extended; but if the rays of light were so bent, they would fall short of the object: besides, if the rays were bent at B, on passing from water into air, a tube bent in the same direction should enable us to see the object; which is never the case. However, it is unnecessary to bring forward more objections than the following experiment.

Exp. 1.—Having placed a piece of money at the bottom of a wine-glass on the table, I made the edge intercept my view; on pouring in a small quantity of water the shilling seemed to rise; I now perceived two images of the object, one at the bottom, and another floating at the top of the water, very apparent when the glass was a little inclined to the eye. This floating image was agitated by every movement of the water. To ascertain whether this image was the real cause of vision, I held the glass above my eye, and saw the image floating by reflection on the surface of the water, as well defined as if reflected from the face of a mirror. Further to convince myself that it is this floating image we see, and not the shilling at the bottom of the vessel, I brought my eye on a line with the image, and then gently lowering the glass, at the same time keeping my eye intently fixed on it, I saw the image by transmitted rays. Thus the floating image was seen by the eye, above, on a line with, and below the water. But it may be objected, If the image were at the surface of the water, why see it on looking into the vessel much deeper than that surface? This objection is answered by analogy with reflecting mirrors; for if we place two candles at different distances, although the images are both evidently formed and reflected from the same surface, yet they appear to the observer at very different distances behind the glass. Let us now draw a few optical inferences from this interesting experiment. 1st. We may infer that when we look through refracting media, such as telescopes, microscopes, spectacles, &c. we take our ideas not from the rays immediately sent from the object itself to the eye, nor from imaginary images at foci, but from images formed in the body of the refracting media. For example: In this experiment, we take our ideas not immediately from the shilling which is covered by the rim of the vessel, but from an image formed perpendicularly over it, at the surface of the water, which, as already mentioned, can be seen by an eye above, below, and on a line, or in the same plane with the surface of the water. 2dly, That there is here no bending: in this experiment the light passes in straight lines from the object at the bottom to the image at the surface, and likewise in straight lines from the image to the eye of the spectator; there is no bending whatever.

Exp. 2.—Having procured from the glass-house a solid glass globe about two inches in diameter. I endeavoured to look through it at the window, but could only perceive a confused light, without any appearance of the frames or window; but on withdrawing my eye a few inches, I saw not only an inverted image of the window, but even the smallest fly became distinct and well defined. Could any person in this experiment venture to say that we take our ideas immediately from the object, and not from the inverted image of that object seeming to float on the posterior surface of the solid sphere? To argue that we could see better at a distance than close to any object, would be absurd. Indeed it is evident from this experiment, as well as from the former, that we take our ideas from the inverted image floating on the posterior surface of the globe, and not from the object, which is as invisible as if it were placed behind our backs. When an oar appears bent in the water, the image of the immersed part is one-fourth nearer to the eye than the rest, consequently it appears bent, or as if broken.

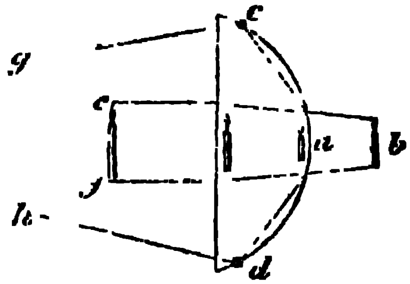
Exp. 3.—When we hold a black pencil or any other substance behind a cylindrical tumbler of clear water, when the pencil is close behind the glass we perceive a magnified image; on withdrawing the pencil to yet a greater distance, this image becomes more and more magnified, and two other images laterally everted are seen at the sides of the tumbler; at yet a greater distance we lose sight of the anterior or magnified image; the two lateral ones floating towards each other, at last form one well defined everted image at the posterior surface, from which, and not immediately from the object, we take our ideas, the object itself being perfectly invisible.

Rays of light diverge, instead of converging, in a convex lens; neither do they cross to *form pictures* of objects, as generally believed.

Should I be enabled to establish these as facts, I strike at the very root of optical science, which I am sanguine enough to believe is likely at no very distant period to undergo as great, if not a greater revolution than the science of chemistry. From the earliest æra, when lenses were first discovered, to the present time, philosophers seeing that on emergence the rays formed a cone, and then crossed, laid it down as an analogical inference, that they also converged in the body of the glass medium. When we find mathematicians measuring the sines of refraction, with a ridiculous accuracy, we cannot help smiling at such waste of time and trouble, when informed by direct and incontrovertible experiments, that nature and the philosophers were travelling very opposite roads. Although every school-boy on looking through his burning-glass, and every old woman through her spectacles, saw objects enlarged; yet the philosophers, instead of repeating the experiments, set about explaining their fanciful theories by the greatest absurdities, and it is looked on as a sort of sacrilege to call in question the opinions of a Newton. However, it should be remembered that in former ages the *αυτος εφη* of Pythagoras was held in equal if not greater estimation, and that it is only within a few years that the *organon* of Aristotle has given place in our colleges to the *novum organum* of Bacon.

Having formed the letter T on a sheet of white paper, I held the plano-convex lens immediately over it, when it appeared considerably enlarged in all its dimensions: on raising the lens about two inches from the paper, two inverted images appeared nearer to the eye, and floating on the posterior surface, forming a kind of circular appearance, in the centre of which the erect image appeared very much enlarged; at yet a greater distance from the eye, the erect image became so diverging and confused as nearly to be invisible, and the two inverted images coalesced and formed in one very distinct inverted image, which diminished in size with every increase of distance. It immediately occurred to me, that this union of these inverted images was the focus of the lens, and consequently that the rays never cross to form pictures. To prove this in the most satisfactory manner, we have only give a circular movement to the lens held over the letter T, and we find the image will become inverted at the top and bottom, erect when at the sides. I next looked through the lens at a lighted candle; when close to my eye it appeared magnified; on slowly withdrawing my eye to about two inches, I perceived two inverted images around the erect one, which formed a brilliant and luminous circle, margined on the outside by bright orange rays, such exactly as we see in the circle of light before the rays are brought to a focus on a sheet of white paper: on now withdrawing my eye to yet a greater distance, I found this luminous circle, formed by these two inverted images, to diminish or contract, and when coalesced, they formed at about two inches and a half from the eye a beautiful inverted image of the candle: as the eye was further removed, this image diminished in size. Here we have two sets of images perfectly distinct from one another and obeying different laws, the erect image magnified, the inverted images diminished by every increase of distance. I now held the lens opposite the lighted candle, and before a sheet of white paper; at the distance of an inch I perceived a luminous circle, margined with orange rays exactly similar to that I saw when looking through the lens at the candle, and formed by the lateral images: on repeating this experiment, any person may be convinced that there is no crossing of rays to form these images, as in fact the inverted image is distinctly seen before the apex of a cone is formed. Further to convince, I shall mention the following conclusive experiment.

Exp. 5.—Having held the glass globe filled with water opposite a *lighted candle*, we find a well defined erect image formed; on placing the plano-convex lens immediately over it, the erect image becomes considerably magnified, and the inverted images are seen forming a luminous circle around; and, as the lens is distanced, they contract or coalesce into one inverted image forming the focus. This may be esteemed an *experimentum crucis*. The following figure may serve to illustrate this interesting subject. *b* a lighted candle; *a*, a small erect image formed on the convex surface of the lens by reflection, transmits the rays *e, f*, to form the magnified image seen by an eye at *e, f*. *c, d*, two inverted images formed by diverging rays striking and reflected from the concave surface, and travelling to *g* and *h*, forming a luminous circle, margined by orange rays. Dr. Herschel justly remarked that the greatest heat was beyond the focus or image; and I have found by repeated experiment that inflammation does not take place at the image of the sun, but on the crossing of the rays. The focus *g, h*, is produced by reflections from *c, d*, and therefore I would suggest the term reflected instead of geometric focus.



When we look at a book through a convex lens, the letters are not only magnified in all their dimensions, but they appear much blacker and better defined, and also much nearer to the eye. How could any bending of the rays produce these effects? The interposition of a semi-transparent substance, such as a glass lens, would undoubtedly diminish instead of increasing the brightness of the letters, if we took our ideas immediately from the object; but on the other hand, when we admit that an image is formed and painted on the posterior surface of the lens, this image being much nearer to the eye would account for the appearance.

I shall not enter into the subject of the identity of heat and light, further than to remark, that the heat is in all probability in a great measure to be attributed to the reflection of the rays from *c* and *d*, and not from any separation of calorific and luminous rays; indeed the discovery already announced in your Journal, and in the experimental outlines, that the prism has a calorific focus, must for ever set at rest Dr. Herschel's speculations; and if I had no further argument in opposition to Sir Isaac Newton's opinions, this fact would be sufficient to convince any unprejudiced reader that the solar ray was never separated by that great man. I am ready to admit that the calorific focus of a convex lens is somewhat removed from the focus of light. The images at *c, d*, form a luminous circle, while the other rays reflected at different angles, according to the angles of incidence, form a calorific focus at yet a greater distance, light not being converted into heat until it is reflected.

In my next I shall endeavour to support Leibnitz's Theory of Refraction, and shall give some new experiments with the prism.

Sir, I remain
Your obedient servant,
Cork, May 24, 1821. J. READE, M.D.

LV. *Process for preventing and correcting an Imperfection in Wines, known by the Name of Ropiness.* By M. HERPIN*.

ROPINESS of wines is a kind of spontaneous decomposition which gives them a consistence similar to that of oil. The wine attacked by ropiness becomes flat and insipid; it turns yellow when poured out, runs in a thread like oil, and loses its natural fluidity. It froths with difficulty by agitation, and disagrees with the stomach. This alteration, which attacks wines during their insensible fermentation, is the more injurious as the alcohol already formed is destroyed to enter into new combinations: ropy wine, therefore, submitted to distillation, gives but a small quantity of brandy, which is of a bad quality, and which has a taste so much the more empyreumatic as the wine distilled is more mucilaginous.

It is remarked that white-wines seldom turn ropy while in cask, but that they do frequently when in bottle.

The remedy for recovering ropy wine, consists in dissolving from six to twelve ounces of acidulous tartrite of potash (cream of tartar), and an equal quantity of coarse sugar, in a gallon of wine heated to boiling. This mixture is to be poured warm into the ropy wine, the cask is to be stopped up and shaken for five or six minutes, and then put in its place with the bung turned downward. After resting for a day or two in that position, the cask is to be turned and the wine fined in the usual way; but instead of stirring it through the bung-hole, as commonly practised, the cask is to be shaken for a few minutes and put in its place with the bung turned up. In four or five days the wine will be clear, dry, limpid, and completely freed from ropiness; but as it cannot safely remain upon the sediment, it must be drawn off, after which it will not be liable to become ropy again. If the ropy wine is in bottles, they should be emptied into a cask, to undergo the preceding operation.

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* From *Bulletin de la Société d'Encouragement.*

LVI. *On* Napier's *Rules of the Circular Parts.* *By* James Ivory, *M.A. F.R.S.*

The illustrious inventor of logarithms applied the whole force of his mind to the shortening of mathematical calculations. Besides his great discovery, he bequeathed to posterity some other contrivances well adapted to the end he had in view. Among these, his two Rules of the Circular Parts, which alone are sufficient for solving all the cases of right-angled spherical triangles, are the most distinguished in point of usefulness.

In our treatises of trigonometry the rules of Napier are represented as enunciations so contrived that, by a particular classification and nomenclature of the parts of a triangle, they include all the propositions necessary for solving every case. They are held up as one of the happiest examples of artificial memory that can any where be found. Rules, entirely dependent on dexterity of arrangement, cannot, it is said, admit of a separate demonstration: they can be proved to be just in no other way than by showing that they comprehend every result, and thus fulfil the purpose for which they were contrived.

In the original tract* in which the rules were first published, the author no doubt demonstrates them by an induction of every possible case. But this mode of proof he was led to adopt, because, not composing an express treatise on trigonometry, it became necessary to show the agreement of the rules with the writings of others. At the close of this demonstration he immediately indicates another and a more general one, which exhibits the whole theory in one view, amounting to this proposition: That two theorems, applied either to the triangle originally proposed, or to one or other of four triangles related to it, comprehend the whole doctrine of right-angled spherical trigonometry. The invention of the circular parts merely enables the author to enunciate the two theorems with reference to the given triangle alone.

It appears therefore that the rules are suggested by real properties of right-angled triangles. The purpose of their inventor seems to have been to reduce trigonometry to the least number of necessary principles, rather than to collect a variety of unconnected particulars into a compendium commodious to the memory. The views of Napier may be applied to abridge the theory of trigonometry, as well as to exhibit its practical precepts in a short abstract. That this is really the case will better appear from what follows.

1. If a great circle of the sphere be described about either of

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* *Mirifici Logarithmorum Canonis Descriptio.*

the oblique angles of a right-angled spherical triangle, as a pole, so as to meet the side opposite that angle and the hypothenuse, both produced if necessary; another right-angled triangle will be formed by the intersections of the three circles, which is said to be *complementary* of the proposed triangle.

Every triangle has two complementary ones, according as the pole of the great circle is made to coincide with one or other of the two oblique angles.

The relations between a triangle and its complementary, are reciprocal. They have a common angle, namely, that which is adjacent to the produced side. The other four parts are the complements of one another; the hypothenuse of one triangle being the complement of the side adjacent to the common angle of the other; and the third side of one, the complement of the remaining angle of the other. It is sufficient here to mention these properties, as the complementary triangle is treated of in all the elementary books[*].

Let h denote the hypothenuse of a right-angled triangle; a and b, the two sides; A and B, the angles opposite to a and b, respectively: then the following Table will exhibit at one view, all the parts of the proposed triangle and its two complementary ones.

		Side.	Angle.	Hypothenuse.	Angle.	Side.
Given triangle.		b	A	h	B	a
Complem' triangles	1	$90-h$	A	$90-b$	$90-a$	$90-B$
	2	$90-A$	$90-b$	$90-a$	B	$90-h$

In this Table the hypothenuses of the three triangles occupy the middle column; and each angle is placed between the hypothenuse and the other containing side.

2. *Theorem* I.— In every right-angled spherical triangle, the rectangle under the radius and the sine of either side is equal to the rectangle under the sine of the angle opposite to that side, and the sine of the hypothenuse.

This proposition is demonstrated in all the elementary treatises[†]. It is no more than an application to right-angled triangles of a general property of all triangles, namely, that the sines of the sides are proportional to the sines of the angles opposite to them.

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[*] See in Playfair's Geometry, *Spher. Trig.* Prop. 20; or in Simson's Euclid, *Spher. Trig.* Prop. 19.

[†] See in Playfair's Geometry, *Spher. Trig.* Prop. 19; or in Simson's Euclid, *Spher. Trig.* Prop. 18.

Now if we apply the theorem to the given triangle and its two complementary ones, we shall get

$$\text{Given triangle.}\begin{cases} R \sin a = \sin h \sin A \\ R \sin b = \sin h \sin B \end{cases}$$

$$\text{1st complem}^{y}\text{ triangle.}\begin{cases} R \cos B = \cos b \sin A \\ R \cos h = \cos b \cos a \end{cases}$$

$$\text{2d complem}^{y}\text{ triangle.}\begin{cases} R \cos A = \cos a \sin B \\ R \cos h = \cos b \cos a. \end{cases}$$

The solution of every case in right-angled trigonometry requires an equation, or proportion, between three parts of the triangle, viz. two given parts and one sought. The total number of equations required for solving all the cases must therefore be 10; for 10 is the number of different ways in which five things can be combined, three and three. Of these 10 equations five have been obtained: and thus one theorem, applied to the given triangle and its two complementary ones, comprehends half the cases that occur in right-angled trigonometry.

In a spherical triangle, the right angle being omitted, Lord Napier gave the name of *circular parts* to the two sides and the complements of the other three parts, namely, of the two angles and the hypothenuse. It is essential that the circular parts be taken in the natural order of their succession in going round the triangle: and hence it is obvious that they are susceptible of no more than five different arrangements. In every arrangement, the two parts next the middle part on the right and left are called *adjacent parts*; and the other two, which stand first and last, are called *opposite parts*. All the possible arrangements of the circular parts may be thus exhibited, each part occupying the middle place successively, viz.

Opposite Part.	Adjacent Part.	Middle Part.	Adjacent Part.	Opposite Part
a	90−B	90−h	90−A	b
b	a	90−B	90−h	90−A
90−A	b	a	90−B	90−h
90−h	90−A	b	a	90−B
90−B	90−h	90−A	b	a

One of Napier's Rules is this:

Rule I.—The rectangle under the radius and the sine of the middle part is equal to the rectangle under the cosines of the opposite parts.

Now the truth of this rule will be established by applying it successively to all the arrangements of the circular parts; for, when this is done, the very same results will be found that have already been obtained by applying the foregoing theorem to the given triangle and its two complementary ones. The two processes are equivalent. Napier's Rule is not only true, but it is sufficient for solving half the cases of right-angled triangles.

3. *Theorem* II.—In any right-angled triangle, the rectangle under the radius and the sine of one side is equal to the rectangle under the co-tangent of the angle adjacent to that side, and the tangent of the other side.

This theorem, in the form of a proportion, is likewise demonstrated in all the elementary treatises of trigonometry*.

If we apply it to the given triangle and the two complementary triangles, we shall get

$$\begin{array}{r} \text{Given} \\ \text{triangle.} \end{array}\left\{\begin{array}{l} R \sin a = \cot B \tan b \\ R \sin b = \cot A \tan a \end{array}\right.$$

$$\begin{array}{r} \text{1st complem}^{y} \\ \text{triangle.} \end{array}\left\{\begin{array}{l} R \cos B = \tan a \cot h \\ R \cos h = \cot A \cot B \end{array}\right.$$

$$\begin{array}{r} \text{2d complem}^{y} \\ \text{triangle.} \end{array}\left\{\begin{array}{l} R \cos h = \cot A \cot B \\ R \cos A = \tan b \cot h. \end{array}\right.$$

These five equations, together with the other five already obtained by means of the first theorem, embrace the whole compass of right-angled trigonometry.

The remaining Rule of Napier is this, viz.

Rule II.—The rectangle under the radius and the sine of the middle part is equal to the rectangle under the tangents of the adjacent parts.

This rule is true; because, when it is applied to all the possible arrangements of the circular parts, it brings out the same five results that have just been found by applying the second theorem to the given triangle and its two complementary triangles. The two Rules of Napier, taken together, are therefore sufficient for solving all the cases of right-angled triangles.

It appears therefore that the whole doctrine of right-angled trigonometry may be brought within the compass of two theorems or rules in two different ways. First, we may employ the two complementary triangles; and then no more is necessary than two of the theorems found in every elementary treatise, without any artificial arrangement or new denominations. The two theorems, applied either immediately to the data in the given triangle, or to the same data transferred to one or other of the two complementary triangles, will solve every case. Or, secondly, we may employ the two rules and the circular parts of Napier. The two methods are fundamentally the same, and differ from one another only in form.

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* See in Playfair's Geometry, *Spher. Trig.* Prop. 18, cor.; or, in Simson's Euclid, *Spher. Trig.* Prop. 17, cor. 2.

In the preceding investigation only three related triangles have been mentioned, whereas the author of the rules employs five. It is to be observed that each of the two complementary triangles has itself a pair of complementary triangles; and as the given triangle is one of each pair, there are no more than two new triangles found in this manner, and these complete the five of Napier. All the five triangles will be exhibited on the surface of the sphere, if each of the two oblique angles of the given triangle be made the pole of a great circle; for the intersections of the two great circles and the three sides of the triangle will form five different right-angled triangles, the hypothenuses of which inclose a pentagonal figure. Every one of the five triangles has its two complementary triangles next it on either hand. The real principles of Napier's Theory consist in these two things: First, all the five related triangles agree in having the same circular parts; secondly, if we take the circular parts of all the triangles, making a similar part always occupy the middle place, we shall obtain all the arrangements of which they are susceptible. Wherefore, since there is the same relation between every triangle and its circular parts, when the two rules are proved, by means of the proper triangle, to be true in any one arrangement, it follows that they must be universally true in every arrangement. The words of Napier, at the close of the demonstration of his rules by induction, are as follows, viz.

"Præter hanc probationem per inductionem omnium casuum, qui occurrere possunt, potest idem theorema lucidè perspici ex 19 et 20, præcedentibus, in quorum schemate, homologa circularium partium constitutio earundem analogiæ similitudinem arguit: ita ut quod de unâ intermediâ et ejus extremis circumpositis, aut oppositis, verè enuntiatur, de cæteris quatuor intermediis et earum extremis respectivè circumpositis, aut oppositis, negari non possit."

The rules of Napier were therefore investigated by means of properties belonging to right-angled triangles. They are a deduction from a theory of considerable subtilty, bearing marks of the same deep and original thinking and profound research, to which we are indebted for the invention of logarithms.

J. Ivory.

LVII. *A Refutation of Mr.* HERAPATH'S *Mathematical Inquiry into the Causes, Laws, &c. of Heat, Gases, Gravitation, &c.*

Is Mr. HERAPATH'S a work, which, like the CRITIC OF PURE REASON, "consists of one chain of closely-connected reasoning, and must therefore be wholly comprehended, or no part of it can be understood?"

Remark of a Disciple of Kant

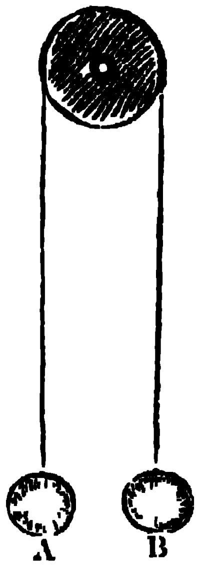
To Alexander Tilloch, LL.D. &c.

SIR, — IN the last Number of the Annals of Philosophy, I am charged with propounding a "new theory of collision," in your Number for August, without demonstration. Whether it be actually new, or not, I have not leisure to inquire; and perhaps it would be less trouble to establish its truth than to seek for other demonstrations to refer to. I am perfectly aware that it is not the same part of the theory of collision as that which generally finds a place in treatises on Mechanics; consequently, I feel a wish to place it before your readers.

The method I propose to take is different from that of preceding writers; but a new view of the subject may be more satisfactory than an old one.

The proposition I have in part to demonstrate, is, that *in the direct collision of perfectly hard bodies the momentum before and after the stroke is the same, when estimated in the same direction.*

Let the bodies be perfectly hard spheres, A and B, and let these bodies be connected by a perfectly inextensible thread, passing over a pulley, so that the movement of the weights is not affected in any manner whatever by the friction, resistance, or inertia of the connecting apparatus.



Case I. Let the ball B be supported at rest, and lift the ball A in a vertical direction, till, when suffered to fall, its velocity at the extent of the thread is V.

Then, because the thread is perfectly inextensible, the velocity will be communicated to B instantaneously and without loss.

The bodies being connected, they would move in the first moment after the stroke with a common velocity $= \frac{AV}{A+B}$; as is proved by writers on Mechanics.

But, if you suppose A to be disengaged in the same instant that it transmits the impulse to B, then the initial velocity of B after the stroke will be $\frac{AV}{B}$.

For, since the active force of A is AV, the intensity of the force on the portion of the thread next to A must be AV; and as the thread is perfectly inextensible, and transmits the force without loss, the impulsive force on B must be AV, and its momentum $\frac{AV}{B}$. Also, since A cannot, on account of the inextensibility of the thread, communicate less than its whole force to B, it would remain at rest unless acted upon by some other force.

Case II.—If both the balls be suffered to fall in the vertical line of the thread, so that at the moment the thread is extended the velocity of A may be V and the velocity of B $= v$,

Then, the bodies being connected, in the first instant of motion after the stroke the bodies will move with a common velocity $= \frac{AV - Bv}{A + B}$; as is proved by writers on Mechanics.

But, if you suppose the bodies to be disengaged, the ins the impulse is communicated, and freed from extraneous then, accordingly as AV, or Bv, is the greater, the velocity will be $\frac{AV - Bv}{B}$, or $\frac{AV - Bv}{A}$.

For, let AV be the greater force, then the tension of the thread cannot be greater than AV, unless there be a reacting force greater than AV; and since Bv is less than AV, the deficiency of reaction is AV$-$Bv. Therefore, AV$-$Bv is the momentum communicated to B; or $\frac{AV - Bv}{B}$ the velocity of B.

As this conclusion does not depend on any particular velocities, it is true when A and B are pressures; and the principle of collision I have propounded is a general principle both in statics and dynamics.

If the momenta AV, Bv, were equal, neither of the balls would move in consequence of the stroke when left at liberty; for AV$-$B$v = 0$.

I have done enough to show any mathematician the difference between the theory Mr. Herapath calls *new* and the old one. I make use of the connecting thread purely for the purpose of assisting Mr. H. in seeing that the intensity of the stroke is the same in Case I. as in Case II. when AV is the same in both. He is well aware that it strikes at the root of his system, as it overturns the conclusions he arrives at in his Prop. V. and Prop. II. Cor. 2. His mistake is, in making the force of contact equal to the sum of the momenta. The force of contact cannot be greater than the momentum of the striking body, for the nature of the opposing force contributes nothing to the stroke, be it reaction or momentum.

I can assure Mr. H. that the rejection of his papers by the Royal Society had no influence with me; if they had been received and printed in the Transactions, I should have equally opposed them. As far however as my opinion goes, the Royal Society do perfectly right in rejecting speculation in physico-mathematical subjects.

I believe Mr. H. when he says he has thought much on the subject: he has not however been happy in finding a correct solution of its difficulties; and if he be guided by the axiom he has laid down at the close of his Reply, I am afraid he will long continue in error. This axiom is, "that it is impossible, by correct reasoning from false principles to bring out true conclusions." The history of science affords too many examples of the evil tendency of a rule like this, even pure mathematics is not an exception. Contemplate the doctrine of a second fluxion passing through *infinity* in the regression of curve lines; examine the de of calculating the radius curvature; review the principle ultimate ratios, and the mathematical doctrine of infinity. Mr. H. must for the present dispense with demonstration; indeed to one that attempts to instruct his brethren in "the physical constitution of the universe," they ought to be unnecessary; I did expect that the bare reference of the laws of collision to a well known dynamical principle would be sufficient; expecting that "a word to the wise is enough;" but should not the explanation I have now given satisfy Mr. H. it will be best to try experiments, or assume, which his axiom will allow him to do, that the atoms are perfectly elastic; and accordingly m[illegible] the world of elastic particles.

I am, sir, your most obedient servant,

No. 2, Grove Terrace, Lisson Grove. THOMAS TREDGOLD.

P. S.—It certainly would have been as well if Mr. Herapath had not published the extracts of letters he has thought proper to do. Private grievances, whether they be real or imaginary, should not be brought forward in the discussions of science. If Mr. H. were desirous of weighing me down with authority, a few extracts from the Chemical Philosophy of Sir H. Davy, from Count Rumford's works, or from Sir Isaac Newton's works, would have been more to the purpose. The existence of an opinion, sanctioned by such names, rendered it necessary to prove, that there cannot be continued intestine motion in bodies consisting of absolutely hard particles.—T. T.

LVIII. *True apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. GROOBY.

[Continued from p. 197.]

1821.	γ Pegasi.	α Arietis	α Ceti.	Aldebaran	Capella	Rigel	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis	Spica Virginis.	Arcturus.
	H. M. 0 4	H M 1 57	H. M. 2 53	H M 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
Dec.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	5·62	10·90	0·57	44·76	35·57	0·93	4 75	33 99	19 60	15 48	0·28	26 42	51·21	53·83	58 69	25·56	49·11	32·24
2	61	89	57	77	59	95	77	34 01	62	51	31	45	24	86	73	59	14	26
3	60	89	57	78	61	96	79	03	64	54	33	48	27	89	76	62	17	28
4	59	89	57	79	63	98	81	05	66	58	36	51	31	93	79	65	20	30
5	58	88	57	80	65	99	82	07	68	61	38	54	34	96	82	69	23	33
6	57	88	57	81	66	1 00	84	08	70	64	41	57	37	54 00	86	72	26	36
7	56	87	57	82	68	01	85	10	72	66	43	60	40	03	89	76	29	38
8	55	87	57	83	69	02	87	11	74	69	46	62	43	06	92	79	34	41
9	54	86	57	84	71	03	88	13	75	72	48	65	46	10	96	82	35	44
10	53	86	57	85	73	04	90	14	77	75	51	68	49	13	99	85	38	46
11	52	85	57	86	74	05	91	16	79	78	53	71	52	16	59 02	89	41	49
12	51	85	56	86	76	06	93	17	81	80	56	73	55	19	06	92	44	52
13	50	84	56	87	77	07	94	19	83	83	58	76	58	23	09	95	47	54
14	49	83	56	88	79	08	96	21	85	86	61	79	61	26	13	99	50	57
15	48	82	56	88	80	09	97	22	86	88	63	81	64	29	16	26·03	53	61
16	47	82	55	89	81	09	98	23	87	91	65	84	67	32	20	06	57	64
17	45	81	55	90	82	10	99	24	89	93	67	86	70	35	23	09	60	66
18	44	80	55	90	83	11	5·00	25	90	96	69	89	72	38	26	13	63	69
19	43	79	55	91	84	11	01	26	92	98	71	91	75	41	30	16	66	72
20	42	79	54	91	85	12	02	27	93	16·00	73	94	78	45	33	19	69	75
21	41	78	54	92	86	13	03	29	95	03	75	96	81	48	36	23	73	78
22	40	77	54	92	87	13	04	30	96	05	77	99	84	51	40	26	76	80
23	39	76	53	92	88	14	04	31	97	08	79	27·01	87	54	43	30	79	83
24	38	76	53	93	89	14	05	32	99	10	82	04	90	57	46	33	83	86
25	37	75	52	93	89	15	06	32	20·00	12	83	06	93	60	50	36	86	89
26	35	74	52	93	90	15	06	33	01	14	85	08	95	63	53	40	89	93
27	34	73	51	93	90	15	07	34	02	16	87	10	98	66	56	43	93	96
28	33	72	50	93	91	15	07	35	03	18	88	12	52·00	69	60	47	96	99
29	32	71	49	93	91	16	08	35	04	20	90	14	03	72	63	50	99	33·02
30	31	70	49	93	92	16	08	36	05	22	91	16	05	75	67	53	50·03	05
31	30	69	48	93	92	16	09	37	06	24	93	18	08	78	70	57	06	08

1821.	1α Libræ	2α Libræ	α Cor Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri	2α Capri	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Dec.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	50 62	1·07	8 39	29 58	29 29	31·19	39 62	53 55	47·22	5 25	33·61	46 05	9 81	21 00	38·44	48·25	54 41	12·99
2	65	09	41	59	30	19	63	54	21	24	60	05	81	20 98	·43	24	40	97
3	67	11	42	61	32	20	63	54	20	24	59	04	80	96	42	22	39	96
4	69	13	44	62	33	20	64	53	20	23	59	04	79	94	41	21	38	95
5	72	16	46	64	35	21	64	52	19	23	58	03	79	92	40	20	37	94
6	74	19	48	66	37	22	65	52	19	23	58	03	78	90	39	18	35	92
7	76	21	49	68	39	23	66	52	18	22	58	02	78	89	38	17	34	91
8	78	24	51	70	41	24	66	52	18	22	57	02	78	87	37	15	33	90
9	82	26	53	72	42	25	67	52	17	21	57	01	77	85	36	14	32	89
10	84	29	55	74	44	27	68	52	17	21	57	01	77	84	35	13	30	87
11	87	32	57	76	46	28	69	52	17	21	56	00	76	82	34	11	29	86
12	90	34	59	78	48	29	70	52	16	20	56	00	76	81	33	10	28	85
13	92	36	61	80	50	31	71	53	16	20	56	00	76	79	32	09	27	84
14	95	39	63	82	52	32	72	53	16	20	56	45 99	76	77	31	07	26	82
15	98	42	65	84	54	34	73	53	16	20	55	99	75	76	30	06	25	81
16	51·01	45	68	87	57	35	74	53	16	20	55	99	75	75	29	05	23	80
17	04	48	70	89	59	37	76	53	16	20	55	99	75	73	29	03	22	78
18	07	51	72	91	61	38	77	53	16	20	55	99	75	72	28	02	21	77
19	10	54	75	93	63	40	78	53	16	20	55	99	75	71	27	01	20	76
20	12	56	77	96	66	41	80	53	16	20	55	99	75	70	26	00	19	75
21	15	59	80	98	68	43	81	53	16	20	55	99	75	68	25	47 98	18	73
22	18	62	82	30 00	71	44	82	53	16	20	55	99	75	67	24	97	16	72
23	21	65	84	03	73	46	83	53	16	20	55	99	75	66	24	96	15	71
24	24	68	87	05	75	48	85	53	17	21	56	99	75	65	23	95	14	70
25	48	72	91	08	78	50	87	54	17	21	56	99	75	64	22	94	13	69
26	31	75	93	10	81	52	88	54	17	21	56	46 00	75	63	21	93	12	67
27	34	78	96	13	84	54	90	55	18	22	56	00	76	62	21	92	11	66
28	37	81	99	16	86	56	92	55	18	22	56	00	76	61	20	91	10	65
29	40	84	9·01	18	89	58	93	56	18	23	57	01	76	60	20	90	09	64
30	43	87	04	21	92	59	95	56	19	23	57	01	77	60	19	89	08	62
31	46	90	07	24	94	61	97	57	19	23	57	01	77	59	19	88	07	61

LIX. Theorems

LIX. *Theorems for the Summation of progressional Series. By Mr.* James Benwell.

To Dr. Tilloch.

Sir, — I have to beg the insertion in your Journal of the following collection of Theorems, being the algebraic expressible sums of the different orders of progressional series respectively enumerated and corresponding therewith. The doctrine and summation of series is a branch of analytics, it is well known, which involves some peculiar difficulties; and the business of investigation requires similar and varied artifices of computation to be employed, since of the methods and principles devised for the purpose, none of them can be so much generalized as to embrace a ready and immediate extension on all occasions indiscriminately. It is chiefly, or rather solely, by deducing the limits and sums of certain orders and forms of series, and by a comparison of the means used, that we are enabled successfully to extend and pursue the same in any new track where the consequences flowing from their composition and resolution are least of all allied and predicable. The theory of series is one likewise not very characterized for the precision of its logic; prejudices in favour and against the metaphysics of it still exist, and prevail with nearly equal force, and these by time alone can be rectified and adjusted.

Now, as preliminary, making $x - 1 = v$, $x^{n-1} = x^m$, n the number of terms of the series to be deduced;—then for the series

$$1 + 2x^{-1} + 5x^{-2} + 8x^{-3} \ldots\ldots (3n-4)x^{-m} \text{ to } n, \text{ terms,}$$

the general expression is $\dfrac{\frac{1}{v}\cdot(2x+1-3x^{-m}) - (3n-1)x^{-m} + v}{v}$, and this by instituting the condition of vanishing quantities in the sense convened usually becomes for the limit of infinity $\dfrac{\frac{1}{v}\cdot(2x+1)}{v}$.

The sum of the series

$$1 + 2x^{-1} + 7x^{-2} + 15x^{-3} \ldots\ldots \tfrac{1}{2}(3n^2 - 5n + 2)x^{-m}$$

is $\dfrac{\frac{3+v}{v}\cdot(x - x^{-m}) + x + v^2 - (3n+1)x^{-m} - \frac{v}{2}(3n^2+n)x^{-m}}{v^2}$.

Here the limit of infinity will arise when all the terms affected by m are feigned practically evanescent, and the same is to be implied of all the succeeding cases.

For the series $1+2x^{-1}+5x^{-2}+9x^{-3}\ldots\ldots\frac{1}{2}(n^2+n-2)x^{-m}$,

$$\frac{\frac{1+2v}{v}\cdot(x-v)^{-m}+v^2-nx^{-m}-\frac{v}{2}(n^2+3n)x^{-m}}{v^2}.$$

Of $1+2x^{-1}+6x^{-2}+12x^{-3}\ldots\ldots(n^2-n)x^{-m}$,

$$\frac{\frac{2}{v}(x-x)^{-m}+2x+v^2-(2n+2+v(n^2+n)x^{-m}}{v^2}.$$

Of $1+2x^{-1}+6x^{-2}+11x^{-3}\ldots\ldots\frac{1}{2}(n^2+3n-6)x^{-m}$,

Is $$\frac{\frac{1+3v}{v}(x-x)^{-m}+v^2-v-nx^{-m}-\frac{v}{2}(n^2+5n-2)x^{-m}}{v^2}$$

Of $1+2x^{-1}+7x^{-2}+14x^{-3}\ldots\ldots(n^2-2)x^{-m}$,

$$\frac{\frac{2x}{v}(x-x)^{-m}+1+v^2-(2n+1)x^{-m}-v(n^2+2n-1)x^{-m}}{v^2}.$$

Of $x^{-1}+6x^{-2}+15x^{-3}+28x^{-4}\ldots\ldots(2n^2-n)x^{-n}$,

$$\frac{\frac{4+v}{v}(x-x)^{-n}-4(n+1)x^{-n}-v(2n^2+3n+1)x^{-n}}{v^2}.$$

Of $1+5x^{-1}+10x^{-2}+16x^{-3}\ldots\ldots\frac{1}{2}(n^2+5n+4)x^{-m}$,

$$\frac{\frac{v^2+2v}{v}(x-x)^{-m}-n(x+2v)x^{-m}-\frac{v}{2}(n^2+n)x^{-m}}{v^3}.$$

Of $1+5x^{-1}+16x^{-2}+33x^{-4}\ldots\ldots(3n^2-4n+1)x^{-m}$,

$$\frac{\frac{6}{v}(x^2-x)^{-m}+v^2-x-(6n+5)x^{-m}-v(3n^2+2n)x^{-m}}{v^3}.$$

Of $1+7x^{-1}+19x^{-2}+37x^{-3}\ldots\ldots(3n^2-3n+1)x^{-m}$,

$$\frac{\frac{5+v^2+v}{v}(x-x)^{-m}+3v-(6n+3)x^{-m}-v(3n^2+3n)x^{-m}}{v^2}.$$

Of $1+2x^{-1}+9x^{-2}+24x^{-3}+50x^{4}\ldots\ldots\frac{1}{2}(n^3-n^2)x^{-m}$,

$$\frac{\frac{3x+x^2-1}{v}(x-x)^{-m}+x^2+v^3-x-(3n+\frac{v}{2}(3n^2+7n+2+v(n^3+2n^2+n)x)^{-m}}{v^3}.$$

Of $1+7x^{-1}+16x^{-2}+27x^{-3}\ldots\ldots(n^2+4n-5)x^{-m}$,

$$\frac{\frac{2}{v}(x^2-x)^{-m}+x^2+5x-(2n+7)x^{-m}-v(n^2+6n)x^{-m}}{v^3}.$$

Of $1+2x^{-1}+10x^{-2}+30x^{-3}+68^{-4}\ldots\ldots(m^3+m)x^{-m}$,

$$\frac{-\frac{6x+2v^2}{v}(x-x)^{-m}+v^3-(6nx^{-m}+v(3n^2+3n)x^{-m}+v^2(n^3+n)x)^{-m}}{v^3}.$$

Of $1+3x^{-1}+16x^{-2}+45x^{-3}\ldots\ldots\ldots(n^3-n^2-n+1)x^{-m}$,

$$\frac{\frac{6+4v}{v}(x^2-x)^{-m}+v^3-vx-(6n+6+v(3n^2+7n+3)x^{-m}-v^2(n^3+2n^2)x^{-m}}{v^3}.$$

Of $1+4x^{-1}+13x^{-2}+26x^{-3}\ldots\ldots\ldots\ldots(2n^2-n-2)x^{-m}$,

$$\frac{\frac{4x}{v}(x-x)^{-m}+1+v^2-(4n+1)x^{-m}-v(2n^2+3n-1)x^{-m}}{v^2}.$$

Of $1+5x^{-1}+13x^{-2}+25x^{-3}\ldots\ldots\ldots(2n^2-2n+1)x^{-m}$,

$$\frac{\frac{4x}{v}(x-x)^{-m}+vx-4nx^{-m}v(2n^2+2n+1)x^{-m}}{v^2}.$$

Of $x^{-n}+3x^{-m}+5x^{-l}\ldots\ldots(2n-1)x^{-1}$,

$$\frac{2nv+x^{-n}+x^{-m}-(x+1)}{v^2}.$$

Of $x^{-n}+2x^{-m}+3x^{-l}\ldots\ldots(nx^{-1})$,

$$\frac{x^{-n}+nv-1}{v^2}.$$

Of $x^{-n}4x^{-m}+9x^{-l}\ldots\ldots\ldots(n^2x^{-1})$,

$$\frac{n^2v+\frac{2}{v}-(2n-1)-\frac{x^{-n}+x^{-m}}{v}}{v^2}=\text{V}.$$

Of $x^{-n}+3x^{-m}+6x^{-l}\ldots\ldots\frac{1}{2}(n^2+n)x^{-1}$,

$$\frac{\frac{v}{2}(n^2+n)+\frac{1-x^{-n}}{v}-n}{v^2}.$$

Of the series $x^{-n}+8x^{-m}+27x^{-l}\ldots\ldots(n^3x^{-1})$,

$$\frac{v^2n(n+1)^2-3\text{V}v^2-(2n^2+n)v^2-3+(3n-1)v+x^{-m}+2x^{-n}}{v^3}=\text{W}.$$

Of $x^{-n}+9x^{-m}+36x^{-l}\ldots\ldots\ldots(\frac{1}{2}(n^2+n))^2x^{-1}$,

$$\frac{\frac{1}{4}(n^2+3n+2)^2-\text{W}-(n+1)^3}{v}.$$

Of $x^{-n}+9x^{-m}+25x^{-l}\ldots\ldots(2n-1)^2x^{-1}$,

$$\frac{v^2(2n+1)^2-8(nv+x)^{-n}x-v^2x^{-n}+8x}{v^3}.$$

Of $x^m+6x^l+21x^k\ldots\ldots\ldots(4n^2-5n)$,

$$\frac{\frac{7x+1}{v}(x^n-1)+vx^n+3v-2vx^m-8n-v(4n^2+3n+2)}{v^2}.$$

Of $x^{-1}+6x^{-2}+18x^{-3}+40x^{-4}\ldots\ldots\frac{1}{2}(n^3+n^2)x^{-n}$,

$$\frac{\frac{3x+v}{n}(v-x)^{-n}+v+v^2-3(n+1)x^{-n}-\frac{v}{2}\left(3n^2+11n+10+v(n^3+4n^2+5n+2)\right)x^{-}}{v^3}$$

The investigation of the foregoing series could not well be given here, either wholly or in an abridged state, as that would necessarily occupy much too considerable space, though it may be remarked that the method adopted and followed is very comprehensive and general in its application. I am, sir,

Your very obedient humble servant,

Aske Terrace. **JAMES B. BENWELL.**

LX. *Proposal for an Apparatus for Flying by means of Motion only.* By A CORRESPONDENT.

To Dr. Tilloch.

SIR, — THE study of aerostation has hitherto been rather a subject of curiosity or amusement than of any real usefulness. The great expense attending the equipment of such an apparatus, as well as the extreme difficulty, next to impossibility, of guiding it through the atmosphere, have all along operated as powerful obstacles in turning it to any useful purpose.

To remedy the latter inconvenience, an ingenious contrivance has been proposed by Mr. Evans, and published in your Magazine*. There are different ways, I believe, in which a balloon might be conducted through the air, were it not for the weight of the materials employed. One of the most obvious would be to have a set of vanes or sails similar to those of a common windmill or smoke-jack attached to a horizontal axis, which being connected with the balloon, and turned swiftly round, would tend to push the whole in the direction of that axis. The huge bulk of the balloon, however, must always encounter much resistance from the air through which it attempts to force its way.

If such an axis with vanes as has just been mentioned, but detached from any balloon, were held in a vertical position and turned round with sufficient velocity in the proper direction, it would, from the reaction of the air, have a tendency to ascend; and if revolving with such a velocity that the disposition to ascend

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[*] Vol. xlvii p. 429.

might just balance its weight, the machine would be suspended in the air. But it is obvious that a *single* vertical axis could not be properly turned by any moving power carried in a vehicle appended from that axis; because such an appendage would soon acquire a rotatory motion in the other direction. This inconvenience, however, might be overcome by having *two* such axes turning in opposite directions; and their bulk again might be materially lessened by making the vanes of the one enter between those of the other, similar to the teeth of two wheels. A more compact form, it is true, would be to have the one axis hollow, and the other within it, with the one set of vanes above the other: but it is easy to perceive that unless the one were far above the other, the upper vanes would establish such a downward current of air on the lower, as to render them useless, or rather hurtful; and indeed it is probable that even in the other form the aëronauts would always have abundance of fresh air blowing downwards about their ears.

Before speculating too far, it may be proper to observe that the whole scheme must still prove abortive, unless the apparatus could be constructed so light that a man, or other portable moving power, might be able to do as much towards turning of the machine as to support himself and his own share of it. Could this be accomplished, I presume the giving it a direction through the air, even against a moderate wind, might be easily effected. For if the weight of the vehicle below, or the centre of gravity, were shifted so that the revolving axes might lean from the perpendicular a little to one side, the whole machine would forthwith endeavour to wing its way toward that side of the horizon; and this apparatus presenting but a small surface to the wind, could withstand or move against it with little resistance compared to that on the inflated side of a bulky balloon.

The difficulty of regulating a balloon so as always to maintain a certain height in the air, as well as the spirit of novelty and adventure, seem to have induced the generality of aërial navigators to keep at a considerable distance, greater perhaps than necessary, above the earth's surface. This circumstance, however, is unfortunately attended with various disadvantages, none of the least of which is the greater rarity of the supporting medium. But the most serious disaster resulting therefrom, is the inevitable destruction of the aëronauts, if by any accident they experience a fall. Besides, should the wind suddenly rise or change, they may, though involuntarily, be wafted to the midst of the ocean, or dashed against the mountain's brow, without either time to count their beads, or bid a final adieu to those they left behind. But the machine just described, could it be made to fly at all, might easily be regulated almost to skim along the surface, and by this means serious falls would be greatly avoided. On the presumption that the machine could thus be guided along any intended track, it might perhaps be practicable to change the men at several stages like coach horses. Indeed I should not despair of yet seeing some such method employed as the most expeditious for conveying the mail from one place to another.

This contrivance is no doubt very inferior to the organs of flight with which the feathered race are furnished, and which enable them to traverse the air with such admirable facility. But it is still a recommendation, that it is free from any reciprocating motion, the vanes obviously acting during every part of their revolution; which is a property entirely wanting in those unfortunate artificial wings contrived to act in imitation of the birds; since such unwieldy wings are not simply useless whilst returning to renew their stroke, but really retard and destroy the flight altogether, as the experiment has uniformly proved.

I have not yet attempted to compute the force to be exerted in supporting such a machine. This would be a task of some difficulty as well as uncertainty; since our best theories of the resistance of fluids are still something short of perfection. It might however, to a certain extent, be compared with the forces acting in the common windmill.

If the above scheme, which is perhaps as plausible as most of the kind that have been proposed, seem to deserve a place in the Philosophical Magazine, the insertion of it will oblige

Yours, &c.

Edinburgh, Sept. 29, 1821. VOLATOR.

LXI. *A Demonstration of* LE GENDRE'S *Theorem for solving such spherical Triangles as have their Sides very small in Proportion to the Radius of the Sphere. By* JAMES IVORY, *M.A. F.R.S.*

THE theorem to be demonstrated is one of singular beauty, and of great usefulness in geodetical calculations. Although many demonstrations have already been given of it, yet the one which follows may merit attention on account of its simplicity.

The theorem is this:

"In a spherical triangle of which the sides are very small relatively to the radius of the sphere, if each of the three angles be diminished by one-third part of the excess of their sum above two right angles, the remainders will be the angles of a plane triangle that has its sides equal in length to those of the spherical triangle."

Let r represent the radius of the sphere, and $a, b, c,$ the three sides of the triangle; then, these four quantities being measured in the same parts, as feet, yards, fathoms, &c. the sides of a similar triangle on the sphere whose radius is unit, will be $\frac{a}{r}$, $\frac{b}{r}$, $\frac{c}{r}$. Suppose that A′, B′, C′, denote the angles opposite to a, b, c respectively; then, because the sines of the sides are proportional to the sines of the opposite angles, we shall have these equations,

$$\left.\begin{aligned}\text{Sin } \frac{a}{r} \sin B' &= \sin \frac{b}{r} \sin A' \\ \text{Sin } \frac{a}{r} \sin C' &= \sin \frac{c}{r} \sin A'.\end{aligned}\right\} \quad (1)$$

Again, in the plane triangle that has its sides equal to a, b, c, let A, B, C, be the angles opposite to those sides; then, because, the sides are proportional to the sines of the opposite angles, we shall have

$$a \sin B = b \sin A$$
$$a \sin C = c \sin A.$$

Suppose
$$\left.\begin{aligned}A' &= A + \delta A \\ B' &= B + \delta B \\ C' &= C + \delta C;\end{aligned}\right\} \quad (2)$$

and, as the angles of one triangle are very little different from those of the other, we may neglect the squares of the small variations: then,

$$\text{Sin } A' = \sin A + \delta A \cos A = \sin A \left(1 + \frac{\delta A}{\tan A}\right)$$
$$\text{Sin } B' = \sin B \left(1 + \frac{\delta B}{\tan B}\right)$$
$$\text{Sin } C' = \sin C \left(1 + \frac{\delta C}{\tan C}\right).$$

Again, $\frac{a}{r}$, $\frac{b}{r}$, $\frac{c}{r}$, being small fractions, we may, with great exactness, suppose

$$\text{Sin } \frac{a}{r} = \frac{a}{r} - \frac{1}{6} \cdot \frac{a^3}{r^3} = \frac{a}{r}\left(1 - \frac{a^2}{6r^2}\right),$$
$$\text{Sin } \frac{b}{r} = \frac{b}{r}\left(1 - \frac{b^2}{6r^2}\right),$$
$$\text{Sin } \frac{c}{r} = \frac{c}{r}\left(1 - \frac{c^2}{6r^2}\right).$$

Now, let these different values be substituted in the equations (1); then,

$$\frac{a \sin B}{r}\left(1 - \frac{a^2}{6r^2}\right)\left(1 + \frac{\delta B}{\tan B}\right) = \frac{b \sin A}{r}\left(1 - \frac{b^2}{6r^2}\right)\left(1 + \frac{\delta A}{\tan A}\right),$$
$$\frac{a \sin C}{r}\left(1 - \frac{a^2}{6r^2}\right)\left(1 + \frac{\delta C}{\tan C}\right) = \frac{b \sin A}{r}\left(1 - \frac{c^2}{6r^2}\right)\left(1 + \frac{\delta A}{\tan A}\right):$$

and, omitting the equal factors on both sides of each equation;

$$\left(1-\frac{a^2}{6r^2}\right)\left(1+\frac{\delta B}{\tan B}\right)=\left(1-\frac{b^2}{6r^2}\right)\left(1+\frac{\delta A}{\tan A}\right),$$

$$\left(1-\frac{a^2}{6r^2}\right)\left(1+\frac{\delta C}{\tan C}\right)=\left(1-\frac{c^2}{6r^2}\right)\left(1+\frac{\delta A}{\tan A}\right):$$

and, by multiplying, and neglecting small quantities of the second order;

$$\left.\begin{aligned}\frac{\delta B}{\tan A}-\frac{a^2}{6r^2}&=\frac{\delta A}{\tan A}-\frac{b^2}{6r^2},\\ \frac{\delta C}{\tan C}-\frac{a^2}{6r^2}&=\frac{\delta A}{\tan A}-\frac{c^2}{6r^2}.\end{aligned}\right\}\qquad(3)$$

Again, in the plane triangle, we have

$$a^2=b^2+c^2-2bc\cos A,$$
$$b^2=a^2+c^2-2ac\cos B,$$
$$c^2=a^2+b^2-2ab\cos C:$$

but, if s represent the area of the triangle, then $2s=bc\sin A=ac\sin B=ab\sin C$: and hence $bc\cos A=\frac{2s}{\tan A}$, $ac\cos B=\frac{2s}{\tan B}$; $ab\cos C=\frac{2s}{\tan C}$. The values of a^2, b^2, c^2 may therefore be thus represented, viz.

$$a^2=\frac{a^2+b^2+c^2}{2}-\frac{2s}{\tan A},$$
$$b^2=\frac{a^2+b^2+c^2}{2}-\frac{2s}{\tan B},$$
$$c^2=\frac{a^2+b^2+c^2}{2}-\frac{2s}{\tan C}.$$

Let these values be substituted in the equations (3); then

$$\frac{\delta B}{\tan B}+\frac{s}{3r^2\tan A}=\frac{\delta A}{\tan A}+\frac{s}{3r^2\tan B},$$
$$\frac{\delta C}{\tan C}+\frac{s}{3r^2\tan A}=\frac{\delta A}{\tan A}+\frac{s}{3r^2\tan C}.$$

and hence,

$$\left.\begin{aligned}\left(\delta B-\frac{s}{3r^2}\right)\tan A&=\left(\delta A-\frac{s}{3r^2}\right)\tan B,\\ \left(\delta C-\frac{s}{3r^2}\right)\tan A&=\left(\delta A-\frac{s}{3r^2}\right)\tan C.\end{aligned}\right\}\qquad(4)$$

Take the sum of these two equations, and of this identical equation, viz.

$$\left(\delta A-\frac{s}{3r^2}\right)\tan A=\left(\delta A-\frac{s}{3r^2}\right)\tan A;$$

then,

$$\left(\delta A+\delta B+\delta C-\frac{s}{r^2}\right)\tan A=\left(\delta A-\frac{s}{3r^2}\right)\left(\tan A+\tan B+\tan C\right).$$

Now, $\delta A + \delta B + \delta C$, is the excess of the angles of the spherical triangle above those of the plane triangle, or above two right angles; and $\frac{s}{r^2}$ is the area of the spherical triangle on the sphere whose radius is unit; and, by the well known theorem of Albert Girard, these quantities are equal. Wherefore

$$\delta A + \delta B + \delta C - \frac{s}{r^2} = 0;$$

consequently,

$$\left(\delta A - \frac{s}{3r^2}\right)\left(\tan A + \tan B + \tan C\right) = 0.$$

Because $A + B + C = 180$, $\tan C = - \tan (A + B)$; therefore, $\tan A + \tan B + \tan C = \tan A + \tan B - \tan (A + B)$, a quantity that in no circumstance can be equal to zero. Wherefore

$$[illegible] = 0;$$

and hence, by equation (4),

$$\delta A = [illegible]$$

$$\delta B = [illegible]$$

$$\delta C = [illegible]$$

Consequently,

$$A = A' - [illegible] = A' - \frac{\delta A + \delta B + \delta C}{3},$$

$$B = B' - [illegible] = B' - \frac{\delta A + \delta B + \delta C}{3},$$

$$C = C' - \frac{s}{3r^2} = C' - \frac{\delta A + \delta B + \delta C}{3}.$$

J. Ivory.

LXII. *On the Change of Colour in Blue vegetable Colours by metallic Salts.* By Mr. J. Murray.

We had rested quietly in the belief that the relations of acids and alkalis to vegetable [illegible]; that the first class of bodies turned [illegible] blues to red, or restored the original tint obliterated [illegible] the second class, or alkalis, restored [illegible] to red by acids, or deepened the yellow [illegible] turmeric, Brasil wood, &c. into brown. [illegible] covered that boracic acid produced the same [illegible] alkalis would do, and I further find that on [illegible], and syrup of violets, this peculiar characteristic is still maintained.

In a series of experiments lately made on vegetable colours, I discovered the remarkable fact, that subacetate of lead, nitrate and sulphate of copper, nitromuriate of platinum, nitromuriate of gold, &c. turned syrup of violets, tincture of cabbage, columbine, blue hyacinth, &c. *green*; and that when these colours are even reddened by acetic or citric or carbonic acid, &c. the metallic solutions *restore the original blue* colour. Boracic acid reddens the yellow colour obtained from *Reseda lut.*, and so do the metallic solutions.

It seems evident, therefore, that we have yet to learn the invariable characteristics of alkalis and acids. We may attempt to cover our ignorance by a free use of the term *anomaly*, but I do hold that in the universe of God there is no such thing as anomaly.

LXIII. *On the Solar Eclipse of the 7th September* 1820; *being a Comparison of Calculations with some of the Observations made in Great Britain and on the Continent. By Mr.* George Innes.

To Dr. Tilloch.

Sir, — In order to compare the observed with the computed times of the phænomena of the late solar eclipse, I have selected several of the observations which have appeared in your Magazine, and in the Edinburgh Philosophical Journal, and made the necessary calculations for the several places by the Tables of Delambre and Burckhardt. From these calculations it appears that the tables give the time of conjunction too early, and the moon's apparent semidiameter too great; as a less semidiameter would have made the errors of the tables, as deduced from the several observations, more uniform for the beginning and end.

The results obtained from the observations of the beginning and end at Gosport, and of the end at Padua, differ much from the rest. Perhaps some error has been committed in allowing for the errors of the clocks, or in transcribing. In calculating the time of the end for Plymouth, I have used the longitude given with the observation; but I observe that it is greater than any of those given in the Requisite Tables, as the result of accurate observations, for eight places in Plymouth.

It is not stated whether the instants of the last five observations are given in mean or apparent time, but from the calculations it would appear that they are given in mean time.

In making the calculations it will be found, that an error of 1″ in the moon's semidiameter gives an error of about 2″,98, and

2″,64 in the times of beginning and end respectively for England, and a little more for the other places, owing to their being in lower latitudes. I would beg leave to request that some of the observers, who, I doubt not, have paid attention to so important a point, would communicate the diameter of the moon, as measured on the sun's disc. The mean of the moon's apparent semidiameters by the calculations made for England is 14′ 50″,73 at the beginning of the eclipse, and 14′ 47″,42 at the end. By the calculations for the other places, it comes out about 1″ greater in both cases. I am, sir, yours respectfully,

GEO. INNES.

In the comparison of the following results of the calculations with the observations, the sign − shows the calculated time to be too early, and + too late.

1. LEIGHTON.

Observed by Mr. Bevan; *mean time.*

	Beginning.	End.
By observation	0^h 18′ 46″	3^h 10′ 28″
By calculation	0 18 1,6	3 10 13,76
	−44,4	−14,24

2. WOOLWICH.

Observed by Mr. Evans; *mean time.*

By obs.	0^h 23′ 2″,85	3^h 14′ 54″,56
By calc.	0 22 29 ,49	3 14 39 ,34
	−33 ,36	−15 ,22

3. BUSHY HEATH.

Observed by Colonel Beaufoy; *apparent time.*

By obs.	0^h 22′ 57″	3^h 14′ 47″
By calc.	0 22 18,55	3 14 35,72
	−38,45	−11,28

4. PLYMOUTH.

Observed by Mr. Fox; *apparent time.*

By obs.		2^h 58′ 56″
By calc.		2 58 19,03
		−36,97

5. GOSPORT OBSERVATORY; *mean time.*

By obs.	0^h 16′ 37″	3^h 10′ 6″
By calc.	0 17 27,71	3 10 26,06
	+50,71	+20,06

6. St. Gall.

Observed by Colonel Scherer.

	Beginning.	End.
By obs.	1^{h} 19′ 8″,05	
By calc.	1 18 31 ,19	
	—36 ,86	

7. Zurich.

Observed by MM. Horner and Feer.

By obs.	1^{h} 14′ 56″,6	4^{h} 3′ 41″,97
and	1 15 0 ,39	4 3 42 ,67
By calc.	1 14 16 ,45	4 3 22 ,22
	—39 ,15	—19 ,75
	—43 ,94	—20 ,45

8. Milan.

Observed by M. Oriani.

By obs.	1^{h} 22′ 7″,5	4^{h} 10′ 48″,7
By calc.	1 21 29 ,59	4 10 33 ,03
	—37,91	—15,67

9. Padua.

Observed by Santini.

By obs.	1^{h} 36′ 20″,6	4^{h} 24′ 53″,3
By calc.	1 35 49 ,3	4 22 47 ,88
	—31 ,3	—2 5 ,42

10. Fiume.

Observed by M. Bouvard.

By obs.		4^{h} 34′ 8″,6
By calc.		4 34 17 ,22
		+8 ,62

LXIV. *Account of a portable Apparatus for restoring the Action of the Lungs.* *By Mr.* John Murray.

"To call the answering spirits back from death."
Byron.

"Forsan scintilla latet."

I fear the *bellows* recommended by the Royal Humane Society for the restoration of the action of the lungs in apparent death, is an instrument but ill calculated for that important purpose. It would, however, not become me to condemn: I rather wish to submit an invention which it is humbly conceived may be used with success.

In No. IX. of the Edinburgh Philosophical Journal is a drawing of the apparatus as perhaps best adapted for houses of recovery. Herewith [see Plate IV. fig. A.] is a sketch taken from a portable form of that invention as executed for me in *Britannia metal*, by Messrs. Dicksons and Smith, of Sheffield. The arrangement is somewhat modified in one transmitted by me to the Royal Humane Society. In this there are *two* belts, at proper distances terminating in screws which fasten by means of nuts to a flat piece of board, and a clamp fixes the whole securely to a square table.

The drawing now submitted exhibits two cylinders concentric with each other, the inner one *three inches diameter*, and the exterior one *four inches*, forming a partition of half an inch between, which is supplied with water heated to 98° F. (the animal temperature), to elevate the air included in the interior cylinder to that grade.

The piston is solid, and moves *horizontally*, and the piston rod *is perforated* to receive a metallic pin, which being checked by the plate covering the end of the cylinder gives us the means of apportioning the volume of air to *the capacity* of the lungs, which is to be determined by the victim of experiment being of tender age or adult. This will obviate the danger of rupturing the lungs.

To the pipe proceeding from this cylinder is affixed a cell and cock, with an elastic tube terminating in a mouth-piece and plate of leather.

The stop-cock is so constructed, that when the handle is parallel with the pipe, as in the figure, there is a free communication established between the lungs and the cylinder, to the exclusion of external air: when, on the other hand, the cock is turned the quadrant of a circle, the communication with the lungs is cut off, and there is a free channel opened between the cylinder and the external atmospheric air.

The lateral cell appended to the cock will be found of varied use and importance. Should the subject of experiment have been the victim of carbonic acid gas (choke-damp), a drop or two of ammonia will mingle with the atmosphere of the cylinder and condense the mephitic gas; and if a septic poison (as sulphuretted or arsenicated hydrogen) have occasioned the asphyxia, a few drops of solution of chlorine or nitromuriatic acid will destroy that septic virus. Should the atmosphere be *too dry*, a small portion of water put into the cell will mix with the air, and impart additional elasticity; and if we desire an additional stimulus, a drop a drop or two of ether posited here will expand in the air of the cylinder, and this mixed atmosphere will act with all the consequence of *nitrous oxide*.

These provisions, for various reasons, are valuable auxiliaries in returning respiration.

The victim of suspended animation is to be raised in a gently inclined position opposite to the operator, the nostrils are to be plugged up, and the plate of leather fixed on the mouth as nearly air tight by means of white of egg, &c. as possible, and this is kept in its position by means of a ribbon tied round the head. The operator over against the victim manages uniformly and equably the piston. The apparatus being adjusted in the manner described, the air is first withdrawn from the lungs and then ejected laterally; and the piston rod being drawn to the extremity of the cylinder (if adult), the pure atmospheric air fills the instrument; and the communication with the lungs being restored by turning the stop-cock parallel with the pipe, the operation begins. About twenty plunges of the piston in the minute may be the proper number; it will not be necessary to change the included air until *natural respiration* is restored, because, until this does take place, the blood cannot eject its excess of carbon, the consequence of the circulation of that fluid; but when this is required, it is instantly accomplished in the manner already described.

It is cheering to me to be assured that this invention has met the most unequivocal testimony of approbation from several eminent physicians and surgeons, and gentlemen of distinguished mechanical genius.

It has been used with great success on some inferior animals.

The internal cylinder, one foot long and three inches diameter, contains 84·82 cubic inches, and about half of this extent will suffice for ordinary respiration, agreeably to the following calculation in Keill's Anatomy: "By the rise of the breast-bone in man and the descent of the diaphragm, room is afforded for 42 cubic inches of atmospheric air at every drawing in of the breath. A deeper inspiration will give room for more than twice this quantity."

The following, deduced from the very interesting experiments of Messrs. Allen and Pepys on respiration, may aid in appreciating the preceding observations.

"1. The inspired air imparts none of its oxygen or nitrogen to the blood.

"2. The blood loses a principle, viz. carbon, which by its union with the oxygen of the inhaled air forms carbonic acid gas.

"3. The watery vapour found in expired air is the serous discharge of the bronchial tubes.

"4. The blood derives heat from the decomposition of the inspired air; all the latent heat of the oxygen gas not being necessary to the formation of carbonic acid gas.

"5. The dark colour of the venous blood is owing to its being surcharged with carbon, and the bright scarlet colour of the arterial blood, to its parting with carbon in the process of breathing."

It may be added, that the volume of the newly formed carbonic acid gas amounts to 4½ to 8 per cent. of the whole elastic mass, which, however, is modified by circumstances, as Dr. Prout has shown: the quantity of carbonic acid gas is diminished, for instance, when mercury or spirits have been used.

It is computed that an ordinary person consumes about 46,000 cubic inches of oxygen *per diem*, and that there are 20 respirations every minute.

By far the most interesting remarks on the elasticity of the lungs, and mechanism of respiration, are contained in an excellent paper by Dr. Carson, published in the Transactions of the Royal Society for 1820, Part I. page 29, &c.

This ingenious author found that a column of water of 1½ foot high, was not a counterpart to the resilience of the lungs of an ox at their usual dilation. In calves, sheep, and large dogs, this elasticity is estimated by the pressure of a column of water from 1 to 1½ foot high; and in rabbits and cats, as counterbalanced by 6 to 10 inches of water.

Breathing, therefore, Dr. Carson very properly ascribes to an interminable combat between the resilience of the lungs and the irritability of the muscular fibre of the diaphragm. We have here displayed to us a simple but beautiful machinery, "by which the heart and diaphragm, and perhaps various other organs, are as necessarily and as effectively influenced as the piston of the steam-engine by the expansive powers of steam."

"Two powers," adds Dr. C., "are therefore concerned in regulating the movements, and in varying the dimensions and form of the diaphragm; the elasticity of the lungs, and the contractile power of the muscular fibres of the diaphragm. Of these powers, the one is permanent and equable; the other, variable, and exerted at intervals. The contractile power of the diaphragm, when fully exerted, is evidently much stronger than its antagonist the resilience of the lungs; but the latter, not being subject to exhaustion, takes advantage of the necessary relaxations of the former, and, rebounding like the stone of Sisyphus, recovers its lost ground, and renews the toil of its more powerful opponent."

Thus has Dr. Carson given us a most interesting account of one of the most important organs of the vital frame. It bears the signet of experiment, and has in its features much that looks like truth. The continuous attacks of the exhaustless, though weaker, assailant rouse the more gigantic, but intermittent, repellent energies of its opponent. It is a war offensive and defensive. Thus does the contest continue for life with equal success, and at its close remain a drawn battle.

J. Murray.

LXV. *An Account of the Comparison of various British Standards of linear Measure.* *By Capt.* Henry Kater, *F.R.S. &c.**

The Commissioners appointed to consider the subject of Weights and Measures, recommended in their First Report "for the legal determination of the standard yard, that which was employed by General Roy in the measurement of a Base on Hounslow Heath, as a foundation for the Trigonometrical operations that have been carried on by the Ordnance throughout the country." In consequence of this determination, it became necessary to examine the standard to which the Report alludes, with the intention of subsequently deriving from it a scale of feet and inches.

On referring to the Philosophical Transactions for 1785, it may be seen in "an Account of the Measurement of a Base on Hounslow Heath," that a brass scale, the property of General Roy (and now in the possession of Henry Browne, Esq. F.R.S.), was taken to the apartments of the Royal Society; and, being there, with the assistance of Mr. Ramsden, compared with their standard (both having remained together two days previous to the comparison), the extent of three feet taken from the Society's standard, and applied to General Roy's scale, was found to reach exactly to 36 inches, at the temperature of 65°.

It afterwards appears that points, at the distance of 40 inches from each other, were laid off on a large plank from General Roy's scale, the whole length being 20 feet; and by means of this plank the length of the glass rods was determined, with which the base on Hounslow Heath was measured.

In the Philosophical Transactions for 1795, it is stated, that Mr. Ramsden compared *his* brass standard with that belonging to the Royal Society, after they had remained together about 24 hours, when "they were found to be precisely of the same length." Brass points were then inserted in the upper surface of a cast-iron triangular bar of 21 feet in length, from Mr. Ramsden's standard, at the distance of 40 inches from each other, the whole length of 20 feet being laid off on those points in the temperature of 54°.

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* From the Transactions of the Royal Society for 1821, Part I.

By means of this bar, the length of the hundred feet steel chain was determined with which the base on Hounslow Heath was re-measured, and was found to be only about 2¼ inches greater than the measurement with the glass rods.

The standard scale used by Mr. Ramsden in laying off the points on the iron bar, is, it seems, no longer to be found; but from the declared equality of both this and General Roy's standard with that of the Royal Society, and the near agreement of the two separate measurements of the base with the glass rods and with the steel chain, one might have been tempted to consider General Roy's scale as precisely similar to Mr. Ramsden's, and as offering the best source from which the national standard yard might be obtained.

The spirit, however, of the recommendation of the Commissioners of Weights and Measures, appearing to be, that the standard yard should be derived *from the base of the Trigonometrical Survey*, I thought it preferable to proceed a step higher, and to obtain a distance of 40 inches from the iron bar itself, which could afterwards be employed in any manner that might be found most eligible.

In order to obviate the necessity of an allowance for temperature, I caused a triangular bar of cast-iron to be made, of the same dimensions as Mr. Ramsden's, except as to length. Gold pins were inserted near the extremities of this bar at the distance of 40 inches from each other, on which were to be drawn fine lines, comprising one-sixth part of the length of the 20 feet bar.

The apparatus used for tracing the lines on the gold pins, is essentially different from that commonly employed. The cutting point is elevated by means of an inclined plane, and is then carried through a distance equal to the length of the line to be traced. On drawing back a part of the apparatus, the extremity of which acts upon the inclined plane, the point descends by its own weight until it wholly rests upon the surface of the bar; the motion being then continued, the frame and cutting point are drawn along together, without the possibility of lateral deviation; and the point describes a line, the length of which may, by a certain contrivance, be regulated at pleasure, and its strength determined by repeating the operation. This very neat and important invention is due to M. Fortin of Paris, and was communicated to me by M. Arago, whose liberal mind knows no reserve on scientific subjects. I have varied the arrangement of M. Fortin, so as to bring the cutting point under a microscope furnished with cross wires, having an adjustment, by means of which their intersection can be brought to the line traced by the cutting point. This I consider to be an essential improvement, as no accidental derangement of the cutting frame can take place without its being immediately perceptible; and the apparatus may be conveniently applied to the division of straight lines or circles, in the manner I have described in the Philosophical Transactions for 1814.

The micrometer microscopes, used in the comparison of the different standards, were those employed in the determination of the length of the seconds pendulum, the description of which may be seen in the Philosophical Transactions for 1818. But as the arrangement of Mr. Ramsden's bar, required that the support to which the microscopes were attached should rest on its surface, some other form of the beam carrying them became necessary for this purpose.

A board was prepared of well seasoned mahogany, 36 inches long, 3 inches wide, and ¼ thick, and an edge bar of mahogany 3½ inches wide and 1¼ thick, was firmly fixed along the middle of it lengthwise, which most effectually prevented the possibility of flexure. To the extremities of this edge bar, and projecting beyond them, the microscopes were fixed, their cross wires being about 40 inches asunder. By this arrangement, the very important advantage was ensured, that the apparatus being laid on a plain surface, such as a scale, and the microscopes adjusted to distinct vision, on placing it on another plane scale, the object glasses of the microscopes would be precisely at the same distance from this last surface as they were from that to which they were applied in the first instance, and consequently no error could arise from parallax.

A piece of very thin brass, usually called latin brass, was bent round the edges of the 40-inch bar, so that the upper surface of the bar was in perfect contact with the brass, the side pressure being just sufficient to prevent any change of position in the brass, unless when moved along the bar by hand. A fine line, about the eighth of an inch long, was now drawn on one of the gold pins at right angles to the bar, and a similar line was traced on the piece of brass, which was placed so as to cover the other gold pin. The intersection of the cross wires of the tracing microscope was carefully adjusted to this last line.

Mr. Ramsden's bar, upon his decease, became the property of Mr. Berge, whose successor, Mr. Worthington, kindly granted me access to it, and facilitated my examination by every assistance in his power. The bar was placed in his workshop on tressels, and its surface carefully brought into the same plane, which was ascertained by stretching a thread from end to end.

The 40-inch bar was laid near Mr. Ramsden's bar, on the 12th of April 1820, and a thermometer placed upon it. Three thermometers were also arranged at equal distances along Mr. Ramsden's bar.

On the 13th of April I commenced my examination. The intersection of the wires of the one microscope being placed on the centre of the left hand dot, the intersection of the wires of the other microscope was brought, by means of its micrometer screw, to the centre of the right hand dot, and the reading of the micrometer registered. In this manner the six intervals of Mr. Ramsden's bar were compared in succession. It may be necessary to remark, that as the microscopes invert, the readings are to be taken in a contrary sense, the higher number indicating defect, and *vice versâ.*

	Readings.	Thermometers.	
1st interval.	29·5	54·0	Ramsden's bar.
2d	10·0	53·5	
3d	10·0	53 5	
4th	16·5		
5th	10·0	53·0	Forty-inch bar.
6th	19·0		
Mean	15·9		

The difference of temperature of the two bars, being so small, may safely be neglected.

The micrometer microscope was now set to 15·9 divisions, and the apparatus being laid on the 40-inch bar, the intersection of the wires of the left hand microscope was brought to the middle of the line on the gold pins, and the piece of latin brass was moved along the bar, till the middle of the line drawn upon it appeared in the intersection of the wires of the micrometer microscope. The whole having been carefully examined, the micrometer microscopes were withdrawn.

The tracing microscope was next brought over the 40-inch bar, and placed so that the intersection of its wires appeared upon the middle of the line traced upon the brass; the brass was then slid away, and a line drawn with the cutting point upon the gold surface.

I had next to compare the distance thus obtained, with the mean of the six intervals on Mr. Ramsden's bar.

First Comparison.

The four thermometers being at 54°, the following readings were taken.

		Readings.		Div.
	Forty-inch bar.	33·5		
Ramsden's bar.	1st interval	54·0	Mean of Ramsden's bar	36·5
	2d	33·0	—— of the forty-inch bar	34·2
	3d	27·0		
	4th	38·0	Forty-inch bar *longer*	2·3
	5th	30·0		
	6th	37·0		
	Mean	36·5		
	Forty-inch bar	35·0		

Second Comparison. Thermometers as before.

		Readings.		Div.
	Forty-inch bar.	36·0		
Ramsden's bar.	1st interval.	54·5		
	2d	31·0	Mean of Ramsden's bar	37·8
	3d	26·0	—— of forty-inch bar	35·5
	4th	40·0		
	5th	35·5	Forty-inch bar *longer*	2·3
	6th	40·0		
	Mean	37·8		
	Forty-inch bar	35·0		

Third Comparison. Thermometers as before.

		Readings.
	Forty-inch bar	35·0
Ramsden's bar.	1st interval	58·7
	2d	30·0
	3d	31·0
	4th	44·0
	5th	34·0
	6th	40·0
	Mean	39·6
	Forty-inch bar	36·2

	Div.
Mean of Ramsden's bar	39·6
—— of forty-inch bar	35·6
Forty-inch bar *longer*	4·0

By the mean of these comparisons, it appeared that the forty-inch bar was *too long* 2·9 divisions of the micrometer, or ·00012½ of an inch *.

The preceding measures were taken from the middle of the lines on the gold pins; but as it was found that these lines were not quite parallel, this accidental circumstance afforded a method, of which I availed myself, to attain a greater degree of accuracy.

The deviation of the two lines was obtained by measuring the difference of the distances of their extremities, and by the mean of six comparisons was found to be 16·8 divisions.

Now, as this is the deviation due to the whole length of the lines, they will have approached each other 2·9 divisions, at about one-sixth part of their length, reckoning from their most distant extremities.

This portion of the line being estimated, transverse lines were drawn, indicating the points from which future measurements were to be taken.

On the 14th of April I resumed my comparisons.

Conceiving that it would be preferable to ascertain the difference between some one interval and the mean of all the intervals of Mr. Ramsden's bar, and afterwards to compare such interval with the forty-inch bar, I now directed my attention to this object.

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* Each division of the micrometer is $\frac{1}{23163}$ of an inch.

Fourth Comparison. *Thermometers* 52°·5.

	Readings.
1st interval	99·0
2d	78·0
3d	73·0
4th	83·0
5th	82·0
6th	83·0
Mean	83·0

15*th April.* *Fifth Comparison.* *Thermometers* 56°

	Readings.
1st interval	107·0
2d	81·0
3d	76·0
4th	89·0
5th	77·0
6th	87·0
Mean	86·1

Sixth Comparison. *Thermometers* 56°.

	Readings.
1st interval	107·0
2d	80·0
3d	79·0
4th	82·0
5th	75·0
6th	83·5
Mean	84·4

On examining the preceding comparisons, it may be perceived that the readings of the sixth interval differ very little from those of the mean of the whole bar.

Readings of the sixth interval.	Mean readings of all the intervals.	Value of the sixth interval + or —.
37·0	36·5	—0·5
40·0	37·8	—2·2
40·0	39·6	—0·4
83·0	83·0	—0·0
87·0	86·1	—0·9
83·5	84·4	+0·9
	Mean	—0·5

The sixth interval, therefore, is too short 0·5 of a division.

This interval was now compared with the forty-inch bar, the thermometers being at 57°; the microscopes were transferred from one bar to the other alternately.

	Readings of the sixth interval.	Readings of the forty-inch bar.
	81·5	85·0
	85·3	85·0
	82·7	86·0
	83·0	83·0
	83·5	83·0
	83·0	82·0
	82·5	82·6
	82·2	82·6
	82·5	81·3
	82·0	81·5
	82·7	82·5
	82·0	81·3
Mean	82·7	83·0

From this it appears, that the forty-inch bar is *shorter* than the sixth interval 0·3 of a division; and as the sixth interval was found to be shorter than the mean of all the intervals 0·5 of a division, the result of the whole is, that the forty-inch bar is shorter than one-sixth of Ramsden's bar 0·8 of a division, or ·000034 of an inch.

I may here remark, that the differences observable between the results of the various comparisons of the intervals of Ramsden's bar, may be attributed to the large size and imperfect state of most of the dots; those bounding the sixth interval are fortunately the least injured.

Having thus obtained the value of the standard, from which the chain used in the Trigonometrical Survey was actually laid off, I next proceeded to compare this with General Roy's and Sir George Shuckburgh's scales.

[To be continued.]

LXVI. *On Shot Cartridges.* *By Mr.* JOSEPH STEEVENS.

To Dr. Tilloch.

SIR,—OBSERVING in your Number for August a paper by A Correspondent in India, on the use of shot cartridges for fowling-pieces; I beg to observe that I adopted the use of such cartridges in the year 1793, and myself and several friends have continued to use them ever since. From the year 1793 to 1804, they were several times taken to India by a friend of mine (who was a Purser in that service): whether he consumed the whole, or disposed of a part with his investment, I know not: certain it is they were not returned to England. Cartridges containing both powder and shot were introduced at the same time, and in many instances have been found very advantageous. And as your Correspondent has omitted to describe the chief utility of shot cartridges, I shall here describe both, and state a few out of many experiments made by myself and others for that purpose.

The chief advantages of shot cartridges are, the prevention of the barrel from leading, and at the same time actually assisting in cleaning it on every discharge. It is well known that in order to make shot bright and handsome, as it is termed, a considerable quantity of black lead is used, a portion of which is at every discharge deposited in the inside of the barrel, and so closely does it adhere that the ordinary mode of cleansing is not sufficient to remove it; and I have within a few days seen a barrel so leaded, as to materially impede its projectile force, divert the shot from its rectilineal course, and deliver it in irregular clusters, leaving spaces near the centre of the charge at forty yards distance, four or five inches diameter; besides which, the lead from the shot, together with the deposit of the powder, actually so contracts the barrel immediately in front of the charge, that (although the gun has been cleaned in the ordinary way) I have known a sound barrel blown to pieces from this cause alone, which the use of shot cartridges would have prevented. Yet I do not consider them indispensable, nor have I adopted them generally, except where a quick succession of discharges is essential. The operation of cleansing at every discharge is thus performed: the cartridge which nearly fits the bore when put in, is enlarged by the explosion of the powder, and, pressing hard against the sides of the barrel, carries before it a considerable portion of the deposit of the last discharge, without allowing the shot to come in contact: thus lessening, instead of increasing, the foulness of the gun, while it renders the leading of the barrel impossible. Giving shot a gloss with black lead is said to be useful as well as ornamental, as it renders it lubricous, and less friction is the consequence: this however holds good only to a limited extent, as the foulness of the barrel by the deposit of the lead soon counteracts the effects of lubricity. In order to discover whether any real advantage was obtained by glazing the shot, I fired fifty charges 2¼ oz. each of No. 4 shot, at a circle three inches diameter, on the balistic pendulum, at fifty yards distance; the gun was cleaned every five discharges: fifty charges 2¼ oz. each of the old patent shot not glazed, were fired at the same pendulum, at the same distance, and with the same gun cleaned after every tenth discharge. The number of shots put in the circle, were with the glazed shot 153; with the old patent 161. Fifty charges of each were then fired in cartridges; the gun cleansed only once in each operation, viz. after the 25th discharge. The numbers were, new patent glazed 159; old patent not glazed 169: the old patent had the advantage in projectile force in both cases, as was obvious by its action on the pendulum. The facility of loading is undoubtedly a material advantage in shooting certain species of wild fowl, as almost all those of the pelican tribe hover over the first bird that is shot; and I have known two persons get four shots each before the flock has dispersed: whereas not more than two each at the utmost, could have been fired in the ordinary way. As to the shot getting into the touch-hole, it so rarely occurs (unless to careless loaders) that it is of no importance.

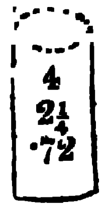
Your Correspondent recommends thin paper. The following experiments will, I apprehend, prove it to be objectionable: Some of the first cartridges I tried were made of whity-brown paper, and others of printing demy, as I had conceived it to be possible that shot confined in thicker paper might go in a lump, and not spread at all. I accordingly made ten cartridges of each of the following sorts of paper, viz. thin whity brown, printing demy, thin post, foolscap, thin blue cartridge, thick ditto, white cartridge, very stout ditto, and very stout brown paper; all of which were well pasted and rolled very close, each filled with 2¼ oz. No. 4 shot: the whole were discharged from the same gun, and not one of them went in a lump: on the contrary, the shot from the thick as well as the thin cartridges spread very similarly. I had then a quantity made by a knife- and razor-sheath maker, of the usual thickness of razor-sheaths; twenty were also made of tin, open at the outer end:—not one of the whole went in a lump, but the spreading of most of those in the paper-cases was irregular, and that in the tin-cases very much so: several of them were deflected from the line of collimation so much as to be useless.

From these as well as a variety of other experiments, I was induced to adopt blue cartridge, white cartridge, and brown paper. I have continued this practice now twenty-eight years, and know of but three instances of the whole charge going in a lump. I have now by me some cartridges upwards of twenty years old. When thin paper is used, the cartridge soon becomes so deformed and enlarged in the middle, as to stick fast in ramming down. Powder and shot cartridges are made similar to those of shot only, the cases being longer. The shot is put in first, then a wad of paper, and then the powder; the end of the cartridge is finally closed over the powder (by the head of the former), which is easily and expeditiously opened when used, the paper being too stout to be bitten off.

I have found those cartridges extremely useful, having with these, as well as with shot cartridges, frequently loaded and fired with effect without drawing the ram-rod. The only objection I know to powder and shot cartridges is, that if not used in a short period of time, say a week or two, or if taken on the water and exposed in a magazine on the deck (which for ready access has usually been the case with myself and friends), the powder becomes materially injured.

Having gone so far, I shall give a sketch of the former, and shape of the paper, &c. with the mode of making and filling; which however is perhaps hardly worth insertion, as it differs not materially from the former, &c. described by your Correspondent.

The paper being folded into ten or twelve thicknesses, and long enough for two, four, or six cartridges, as A B fig. 1, a tin or wood pattern *a b c d* is applied, and with a knife the paper is cut through; the corners *e e* are cut off, being objectionable in the formation of the cartridge; the paper must be sufficiently large, that, when rolled on the former, the top *a b* shall reach once and half, and the bottom *c d* twice round. The former is about 6 inches long, having a head about 1¼ inch diameter, as represented in fig. 2. In rolling the cartridge, the former must fall short of the bottom about 2-3rds of the diameter, to allow for closing. When the cartridges are dry, they are again forced on the former, and their bottoms again pressed on the closing nail driven in the rolling-board for that purpose. The cases are now placed in a block of wood having two or three dozen holes like a cartridge-box, and with a funnel and measurer filled very expeditiously, the upper ends are closed and hammered in with the head of the former. The number expressing the size of the shot, the weight of the charge, and the diameter of the barrel for which they are intended, are then marked on the cartridge thus: i. e. No. 4 shot 2¼ oz. for barrel ·72 diameter. This is essential, as some cartridges remain in stock many years, and, although promiscuously mixed together, are easily separated.

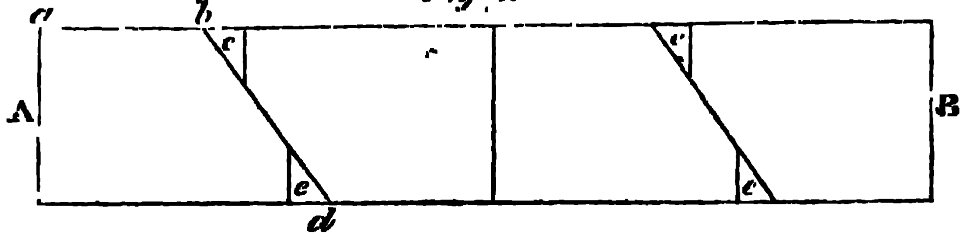


If the above occupies too much space to be admitted at length, please to make such extracts as you think fit.

While on the subject of fowling-pieces, I beg leave to remark, that although numerous excellent sporting guns are produced; yet no scientific principle has been laid down, on which to proceed with a certainty of obtaining this desirable end. From a variety of experiments, I am induced to believe there is a certain ratio between the length of the barrel and diameter of the bore, which gives the maximum of perfection. Perhaps some of your scientific readers will favour the public with some observations on this subject. I am, sir, your obedient servant,

Old Ford, Oct. 12. 1821. Jos. Steevens.

Fig. 1.



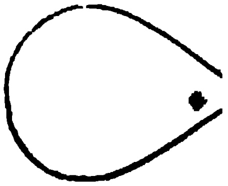


Fig. 2.

Section of the Former.

LXVII. *Remarks tending to facilitate the Analysis of Spring and Mineral Waters.* By John Dalton *.

It cannot but fall under the observation of every one, that the health and comfort of families, and the conveniences of domestic life, are materially affected by the supply of that most necessary article, water. The quality of water is undoubtedly of great importance in the arts of brewing, baking, and various others connected with the preparation of food; as also in the washing and bleaching of linen and cotton, and in other operations where cleanliness is the object in view. Many of the manufactories are materially interested likewise in the qualities of water, and in the methods of rendering it subservient to their exigencies when it happens to be presented to them in an obnoxious form. On all these accounts I thought it might be of some service to offer a few remarks on the subject, which, perhaps, may benefit those who have not made the science of chemistry a peculiar object of study.

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* From the Memoirs of the Literary and Philosophical Society of Manchester.

Most writers consider the analysis of waters as a problem requiring great skill and acquaintance with chemistry; but the modern improvements in that science have rendered it much less so than formerly. It is true, that the variety of elements sometimes found in water, and the extremely small quantities of them, are discouraging circumstances when the object of analysis is to ascertain both the *kind* and *quantity* of these foreign elements. They may both, however, be investigated without much labour, when proper means are used; and, perhaps, a little practice may render a person qualified to undertake the task, who is no great adept in chemical science in general.

Most spring water that is obtained by sinking some depth into the earth, contains lime held in solution by some one or more acids, particularly the carbonic and sulphuric acids.

It is to these salts, the carbonate and sulphate of lime principally, that spring water owes its quality of *hardness*, as it is called; a very singular and astonishing quality, when it is considered as produced by so extremely small a portion of the earthy salt. The other earthy salts, or those of magnesia, barytes, and alumine, produce the same effect nearly, but they are rarely met with, compared with those of lime.

When any earthy salt is dissolved in pure distilled or rain water, it increases the specific gravity of the water; but, in the instance of spring water in general, this test is rendered of little use, because the increase of sp. gr. is so small as almost to elude the nicest instrument that can be made. I have, however, an instrument, made by an artist in this town, which is nothing more than the common glass hydrometer, but with an unusually fine small stem, that shows the superior gravity of spring water. It cannot, indeed, be brought in competition with other methods for ascertaining the relative hardness of spring water, but it is a most useful instrument in other departments of chemical investigation, particularly in determining minute portions of residual salt after precipitations *. It may well be conceived, that the sp. gravity cannot constitute a test of the hardness of water, when we find that *one grain* of earthy salt, dissolved in 2000 grains of pure water, converts it into the hardest spring water that is commonly found.

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* The scale of the hydrometer is one inch and a half long, and it is divided into 25°, each degree corresponding nearly to ·0004; the difference between distilled water and common spring water is usually about 1° on the instrument; and that between distilled or rain water and the strongest lime water is 4°.

We shall now proceed to notice some of the most useful tests in the analysis of waters.

1. *Soap Test.*—When a piece of soap is agitated in distilled or pure rain water, a part of it is dissolved, producing a milky liquid, which continues for many days unaltered. But when soap is agitated with hard spring water, the milkiness produced almost instantly degenerates into a curdy substance, which rises to the surface, and leaves the liquid below nearly transparent. This curdy substance is understood to be the earth of the salt combined with the oil of the soap. It has a glutinous, unpleasant feel when rubbed upon the hands, and soils glass and other vessels so as to require hard pressure of a cloth to remove it. Though this test sufficiently distinguishes hard water from soft or pure water, it is not equal to form an accurate comparison of the hardness of two kinds of water.

2. *Lime-water Test.*—Most spring water, fresh from the well, will exhibit milkiness by lime-water; this is usually occasioned by the water holding supercarbonate of lime in solution; the addition of lime-water reduces the supercarbonate to carbonate, which is insoluble, and falls down in the state of a white granular powder. When a spring contains nothing but supercarbonate of lime, which is the case with the water of an excellent pump in this neighbourhood, lime-water is the only test wanted to ascertain the proportion of salt in it. Let a given portion of the spring water be saturated by lime-water, adding it as long as milkiness ensues; the carbonate of lime is precipitated, and may be determined by the usual means. I find it, however, rather preferable to add a small excess of lime-water to secure the precipitation of the whole acid: when the salt has subsided, the clear liquid may be poured off, and tested by an acid, and the salt may be dissolved by test muriatic or nitric acids. Thus the whole quantity of lime will be found; from which, deducting that added in lime-water, there will remain the lime in the spring water originally combined with the carbonic acid. In this way I find the supercarbonate of lime, in five ounces of the water above mentioned, to consist of

consist of	.48 lime,
	·77 carb. acid,
	1·25

being about one grain of salt in 2000 of water. This kind of water is hard, and curdles soap; but it is much softened by boiling, and deposits the incrustation so often found in kettles, &c.

If water contains sulphate of lime along with supercarbonate, the same treatment may still be adopted as far as respects the supercarbonate. I have recently found, with some surprise, that the surpercarbonate of lime, as I call it, existing in waters, or made artificially, is rather an *alkaline* than *acid* compound.

3. *Acetate and Nitrate of Lead Tests.*—These salts are easily obtained in great purity, and are excellent tests for carbonic and sulphuric acid, which they precipitate immediately in combination with the lead. If the precipitate be treated with nitric acid, the carbonate of lead is instantly dissolved, and the sulphate of lead (if present) remains undissolved, and may be collected and dried; from which the quantity of sulphuric acid may be determined.

4. *Nitrate and Muriate of Barytes Tests.*—When the object is to ascertain the presence of sulphuric acid, either free or combined, these are the best tests. The sulphate of barytes is perhaps the most insoluble salt known. Even rain water collected from slated houses, though softer than spring or river water, exhibits by these tests one grain of sulphuric acid in 20 or 30 thousand grains.

5. *Oxalic Acid Test.*—When the object is to obtain the lime, either free or combined, in any water, this is the best test. It may be proper to add a little ammonia in some cases of combined lime. The oxalate of lime slowly precipitates in the state of an insoluble salt. The quantity of lime may be ascertained, either by collecting the precipitate, or by carefully and gradually adding the due quantity of acid and no more, when the strength of the acid has been previously ascertained.

6. *Nitrates of Silver and Mercury Tests.*—These are tests of muriatic acid or of muriates; the muriates of silver and mercury are formed, both insoluble salts. It does not often happen that spring waters contain notable proportions of the muriatic acid either free or combined.

7. *Sulphuretted Hydrogen-water and Hydro-sulphurets.*—These are excellent tests for lead, mercury, and several metals, giving peculiar insoluble precipitates of the sulphurets of those metals. One grain of lead precipitated by sulphuretted hydrogen, would be sufficient to give a great many gallons of water a dark brown tinge. When sulphuretted hydrogen is found in mineral waters, as those of Harrowgate, it may be known by the smell; but solutions of lead are much superior tests, giving a black or brown tinge to such waters immediately.

8. *Tincture of Galls and Prussiates of Potash and Lime Tests.*—These are proper for the detection of iron, the former giving a black precipitate and the latter a blue one; but a portion of the solution of oxymuriate of lime requires to be added pre-

viously to the water, if it contains the green oxide of iron in solution, in order to convert it to the red oxide.

There are many other tests than those I have enumerated, but they are more than can usually be wanted in the analysis of ordinary springs. My object is not to give a catalogue of tests, but to show in what manner their application may be improved, and reduced to a system intelligible to moderate proficients.

The improvements I would propose in the use of tests are, that the exact quantities of the ingredients in each test should be previously ascertained and marked on the label of the bottle; this might easily be done in most of them in the present state of chemical science. We should then drop in certain known quantities of each from a dropping tube graduated into grains, till the required effect was produced; then from the quantity of the test required, the quantity of saline matter in the water might be determined without the trouble of collecting the precipitate; or, if this was done, the one method would be a check upon the other.

I shall now close this imperfect sketch by a few observations and experiments which I have noticed in the course of the present week relative to the subject before us.

I assayed the water supplied by the Manchester water-works, and found it nearly as I expected; river water is most commonly softer than spring water, and harder than rain water. This is the case with the water in question. It contains a very little sulphate of lime and some carbonate; but only one half of the earthy matter that the above-mentioned pump water contains. It curdles a little with soap, but gives no precipitate with lime-water. It contains about 1 grain of earthy salts in 4000 of water.

When spring water contains supercarbonate of lime, boiling it precipitates the greater part of the carbonate, and expels the excess of acid. Hence the *furring* of pans and tea-kettles with this kind of water. By boiling the water it is of course rendered much softer than before. It may then be used for washing, scarcely curdling soap; but it still contains about $\frac{1}{3}$ of the earthy salt, and gives milkiness with acetate of lead. If a water contain only sulphate of lime, boiling does not, I apprehend, soften it at all.

When spring water is used by manufacturers for washing, &c. it is advantageous to have it some time exposed to the atmosphere, in a reservoir with a large surface. This exposition suffers the carbonic acid in part to escape, and the carbonate of lime to precipitate; and in some degree supersedes the necessity of boiling the water. The more any spring is drawn from, the softer the water becomes, it should seem. I have this morning examined a spring which yields many thousand gallons every day. The water is comparatively soft; it does not curdle scarcely at all with soap: it is very nearly as soft as the before-mentioned pump water boiled. The hardness in it arises from a little sulphate of lime and a little carbonate.

One of the most striking facts I have observed is, that all spring water containing carbonate or supercarbonate of lime, is essentially *limy* or alkaline by the colour tests. And this alkalinity is not destroyed till some more powerful acid, such as the sulphuric or muriatic, is added, sufficient to saturate the whole of the lime. Indeed these acids may be considered as sufficient for tests of the quantity of lime in such waters, and nothing more is required than to mark the quantity of acid necessary to neutralize the lime. It does not signify whether the spring water is boiled or unboiled, nor whether it contains sulphate of lime along with the carbonate; it is still *limy* in proportion to the quantity of carbonate of lime it contains. Agreeably to this idea, too, I find that the metallic oxides, as those of iron or copper, are thrown down by common spring water just the same as by free lime. Notwithstanding this, carbonate of lime in solution with water contains twice the acid that chalk or limestone does. I fully expected the supercarbonate of lime in solution to be *acid*. But it is strongly alkaline, and scarcely any quantity of carbonic acid water put to it will overcome this alkalinity. Pure carbonic acid water is, however, *acid* to the tests. I could not be convinced of the remarkable fact stated in this paragraph, till I actually formed supercarbonate of lime, by supersaturating lime-water in the usual way, till the liquid from being milky became clear. It still continued limy, and was even doubtfully so when two or three times the quantity of acid was added. It should seem, then, to be as impossible to obtain a *neutral* carbonate of lime, as it is to obtain a *neutral* carbonate of ammonia in the sense here attached to the word neutral.

LXVIII. *Notices respecting New Books.*

Preparing for Publication.

An Essay on the Strength and Flexibility of Cast Iron; with Practical Rules and Tables for various Purposes in Engineering and Architecture; Remarks on the Forms of greatest Strength; and, an Account of some new Experiments on the Strength of Iron. By Thomas Tredgold, Author of "Elementary Principles of Carpentry," and of the Article JOINERY in the New Supplement to the *Encyclopædia Britannica.*

Mr. Gill, for many years one of the Chairmen of the Committee of Mechanics in the Society for the Encouragement of Arts, Manufactures, and Commerce, in the Adelphi, assisted by a circle of Mechanical Friends in this and other Countries, is preparing for Publication, A Technical Repository of Practical Information on Subjects connected with the present daily Improvements and new Discoveries in the useful Arts.

From this Gentleman's extensive knowledge and connexions, we have every reason to expect a fund of valuable information. The First Part is promised in January of the ensuing year.

Miscellaneous Works of the late Robert Willan, M.D. F.R.S. F.A.S., comprising an Inquiry into the Antiquity of the Small-pox, Measles, and Scarlet Fever; now first published.—Reports on the Diseases in London; a new Edition, &c. in one volume, octavo. Edited by Ashley Smith, M.D., Licentiate of the Royal College of Physicians of London.

An Account of the Interior of Ceylon, and of its Inhabitants. With Travels in that Island. By JOHN DAVY, M.D. F.R.S. 4to. pp. 530. London, 1821.

With the exception of the 'Historical Relation of the Island of Ceylon,' by Robert Knox, an English seaman, who was wrecked on the coast in 1660, and suffered twenty years' captivity, there is not a single volume in existence on the natural or civil history of Ceylon. Knox's History has always been popular, on account of its simplicity of style and narrative, and the good sense and good feelings of the author; but his sphere of observation was necessarily limited, and the period of one hundred and forty years, which has elapsed since its publication, and the vast interest which the subject has acquired since Ceylon has become a British province, have rendered an account of it one of the most acceptable works that could be offered to the public.

The present work has been drawn up by Dr. Davy from original materials collected in Ceylon during a four years' residence of the author, who was on the medical staff of the army. He has received the assistance of every one who was capable of aiding him in the information, and hence the work is enriched by many valuable contributions. It gives a full account of the history, geography, and geology of the island; its population, laws, language, and religion; the state of the arts and sciences, the domestic habits and manners of the inhabitants, &c. A work of this nature, written with the ability which Dr. Davy has displayed, cannot fail of exciting great interest; and to those parts which fall more particularly within the scope of our work, we purpose briefly to direct the attention of our readers.

The Island of Ceylon is in the tropic of Cancer, situated nearly between the parallel of 6° and 10° north latitude, and between

80° and 82° east longitude; that is, at the western entrance of the Bay of Bengal, and off the coast of Coromandel. It is almost two-thirds of the size of Ireland, containing altogether a surface of 27,770 square miles, and a population of about 800,000 souls, which is in the proportion of about thirty-eight only to a square mile.

The character of the interior, as to surface, greatly varies: it may be divided into flat country, hilly, and mountainous; the latter district, in perpendicular elevation above the sea, varies from 800 to 3000, and even to 4 and 5000 feet. There are no lakes, not even a single stagnant pool among the mountains. Uniformity of formation is the most remarkable feature in the geological character of Ceylon, the whole of which, with very few exceptions, consists of primitive rock, the prevailing species of which is granate or gneiss.

The soil of the island is generally poor, but it abounds in rivers and springs; the proportion of rain that falls in it is very great, exceeding three or four times what falls in England. In respect to heat or temperature, no tropical country is, perhaps, more favoured than Ceylon; its hottest weather being temperate, in comparison with the summer heats of most parts of the continent of India. Generally speaking, the climate is salubrious.

The mineralogy of Ceylon is singular and curious; it is remarkable for its richness in gems, and its poverty in the useful metals. It is remarkable, too, for the number of rare minerals that it affords, and for the small variety of the ordinary species; being thus, in its mineralogical character, quite oriental, better fitted for show than utility—for pomp than profit*. The principal gems are the ruby, garnet, topaz, amethyst, sapphire, and rock crystal.

Dr. Davy has bestowed little attention on the botany of Ceylon, and treats very briefly of its animals (which do not differ from those on the adjoining continent of India); yet he has paid particular attention to the snakes of the island, which are neither so numerous nor so dangerous as they have been represented. Our author collected twenty different species of snakes, of which sixteen were harmless. Of those that are poisonous, the *Pimberah* is the most remarkable. It is characterized by its great size, and by a couple of horny probosces, in form and curvature not unlike the spurs of the common fowl; the base of the spur is attached to a small bone, with a minute head, which is received into the glenoid cavity of a thin long bone, that terminates in a tapering cartilaginous process. These horny spurs are useful in enabling the snake to climb trees and hold fast its prey.

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* The only metallic ores hitherto found in Ceylon are of iron and manganese.

"This snake (says the author) is the largest species in Ceylon; and the only one that grows to a great size. I have seen a specimen of it about seventeen feet long, and proportionably thick. It is said by the natives to attain a much greater magnitude, and to be found occasionally twenty-five and thirty feet long, and of the thickness of a common-sized man. The colour of different specimens that I have seen has varied a little: it is generally a mixture of brown and yellow; the back and sides are strongly and rather handsomely marked with irregular patches of dark brown, with dark margins. The jaws are powerful, and capable of great dilatation; and they are armed with large strong sharp teeth reclining backwards. As the muscular strength of this snake is immense, and its activity and courage considerable, it may be credited that it will occasionally attack man; there can be no doubt that it overpowers deer, and swallows them entire.

"The natives have many ridiculous stories respecting this snake. They say, that when young, it is a polonga, and provided with poisonous fangs; and that when of a certain age and size it loses these fangs, acquires spurs, and becomes a pimberah. They suppose its spurs are poisonous, and that the animal uses them in striking and killing its prey. They imagine that parturition is always fatal to the female, owing to the abdomen bursting on the occasion; and that the males, aware of this circumstance, out of regard for the females of their species, avoid them, and choose for their mates female noyas."

The most common of the poisonous snakes of Ceylon, is the Noya or hooded snake of the English, and *Coluber nuja* of Linnæus. The natives rather venerate this snake than dread it, and will not even kill it when found in their houses.

"Frequent exhibitions are made of this snake in Ceylon, as well as on the continent of India, by men called snake-charmers. The exhibition is rather a curious one, and not a little amusing to those who can calmly contemplate it. The charmer irritates the snake by striking it, and by rapid threatening motions of his hand; and appeases it by his voice, by gentle circular movements of his hand, and by stroking it gently. He avoids, with great agility, the attacks of the animal when enraged, and plays with it and handles it only when pacified, when he will bring the mouth of the animal in contact with his forehead, and draw it over his face. The ignorant and vulgar believe that these men really possess a charm, by which they thus play without dread and with impunity, with danger. The more enlightened, laughing at this idea, consider the men impostors, and that in playing their tricks there is no danger to be avoided, it being removed by the extraction of the poison-fangs. The enlightened in this instance are mistaken, and the vulgar are nearer the truth in their opinion. I have examined the snakes I have seen exhibited, and have found the poison-fangs in, and uninjured. These men do possess a charm, though not a supernatural one, viz. that of confidence and courage: acquainted with the habits and disposition of the snake, they know how averse it is to use the fatal weapon Nature has given it for its defence in extreme danger, and that it never bites without much preparatory threatening. Any one possessing the confidence and agility of these men, may irritate them, and I have made the trial more than once. They will play their tricks with any hooded snake, whether just taken or long in confinement, but with no other kind of poisonous snake."

Dr. Davy made several experiments on the poison of the snakes; whence he concludes that there are only two snakes at Ceylon, the hooded snake and the tic-polonga, whose bite is likely to prove fatal to man.

There is another animal in Ceylon, less dreaded but much more troublesome, and the cause of the loss of more lives than the snakes. This is the Ceylon leech:—

'This animal varies much in its dimensions; the largest are seldom more than half an inch long, in a state of rest; the smallest are minute indeed. It is broadest behind, and tapers towards the forepart; above, it is roundish; below, flat. Its colour varies from brown to light brown; it is more generally the latter, and rarely dark brown. It is marked with three longitudinal light yellow lines, extending from one extremity to the other; one dorsal and central, two others lateral. The substance of the animal is nearly semi-transparent, and, in consequence, its internal structure may be seen pretty distinctly. A canal appears to extend centrically the whole length of the body, arising from a crucial mouth at the smaller extremity, and terminating in a small circular anus at the broader extremity, on each side of which are two light spots.

"This leech is a very active animal; it moves with considerable rapidity; and it is said occasionally to spring. Its powers of contraction and extension are very great; when fully extended, it is like a fine cord, and its point is so sharp that it readily makes its way through very small openings. It is supposed to have an acute sense of smelling; for no sooner does a person stop where leeches abound, than they appear to crowd eagerly to the spot from all quarters.

"This animal is peculiar to those parts of Ceylon which are subject to frequent showers; and, consequently, it is unknown in those districts that have a long dry season. It is most abundant among the mountains,—not on the highest ranges, where the temperature appears to be too low for it, but on those not exceeding two or three thousand feet above the level of the sea. It delights in shady damp places, and is to be seen on moist leaves and stones more frequently than in water. In dry weather it retires into the close damp jungle, and only in rainy weather quits its cover, and infests the pathways and roads and open parts of the country.

"Whether it is found in any other country than Ceylon is not quite certain; perhaps the leech of the mountainous parts of Sumatra, noticed in Mr. Marsden's History of that island, is similar to it; and it is not unlikely that it occurs amongst the damp and wooded hills of the south of India. Those who have had no experience of these animals, of their immense numbers in their favourite haunts, of their activity, keen appetite, and love of blood, can have no idea of the kind and extent of annoyance they are to travellers in the interior, of which they may be truly said to be the plague. In rainy weather, it is almost shocking to see the legs of men on a long march, thickly beset with them gorged with blood, and the blood trickling down in streams. It might be supposed that there would be little difficulty in keeping them off: this is a very mistaken notion, for they crowd to the attack, and fasten on, quicker than they can be removed. I do not exaggerate when I say that I have occasionally seen at least fifty on a person at a time. Their bites, too, are much more troublesome than could be imagined, being very apt to fester and become sores; and, in persons of a bad habit of body, to degenerate into extensive ulcers, that in too many instances have occasioned the loss of limb, and even of life."

In the sciences the Singalese have made scarcely any progress; but in the arts, particularly those of an ornamental kind, their attainments are considerable. Of these, Painting is the least advanced; for they are still without any knowledge of perspective. In Statuary they have been more successful. As in Ancient Greece, their religion offers a never-failing subject, and every temple affords employment. *Boodhoo* is the common subject of their statuaries, and figures of him of all sizes are to be seen in their temples. In the art of Casting, too, the Singalese exhibit considerable skill. Their taste is however best displayed in their jewellery, which would be admired even in this country, and, Dr. D. thinks, not very easily imitated.

It is generally remarked, that the ruder the method employed in any country for the reduction of iron, the better the quality of the metal is. The observation holds good in Ceylon; their process of smelting iron is remarkable for its simplicity. The most complete Singalese smelting-house which Dr. D. had an opportunity of seeing, consisted of two small furnaces under a thatched shed. The Singalese blacksmith is in the exercise of his art far from being unskilful; he is perhaps (says Dr. D.) on a par with the common country blacksmith in any part of Europe.

The preparing of saltpetre and the manufacture of gunpowder are arts which the Singalese have for many years constantly practised According to their own account, they first learnt from the Portuguese the use of fire-arms and the art of making them, and of manufacturing gunpowder, of both which they were completely ignorant before they had intercourse with Europeans.

LXIX. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF CORNWALL.

At the Third Annual Meeting of this Institution, on the 27th of August last, the Council reported that the Museum has been enriched by many valuable presents during the past year; they notice particularly the present of an Egyptian Isis from the noble President, Viscount Exmouth. This statue was for a vast number of years in the family of Elfi Bey, and presented by the British Consul at Alexandria to His Lordship.

The Council have recommended to the Society the appropriation of a room for the exhibition of paintings and drawings, to which artists should be invited to send their productions. And at another Anniversary they hope the Society will be able to carry into effect a plan for offering premiums for papers on literary or scientific subjects, or for improvements in the various arts and manufactures of the County. It will, they conceive, be attended with very advantageous effects to the Institution, and, they flatter themselves, prove eventually beneficial to the County: if in no other way, by calling forth talent, latent only for want of a stimulus to excite it to exertion.

From the members of the Institution the Council hope that many original communications may be expected, on the numerous interesting phænomena which this County exhibits. To the admirers of Chemistry a very wide field is presented by the numerous minerals of this County, many of which have never been analysed, and others only hastily, and at a distance from their localities.

In concluding their Report, the Council made a strong appeal to the County at large on behalf of an Institution which embraces so wide a sphere of usefulness. "Shall Cornwall," they say, "that part which, of all the British dominions, depends most upon the practical application of science for the successful prosecution of its important interests, be the most backward in support of an Institution whose principal object is the diffusion of Science? To the Miner, what can be of more importance than that knowledge which may eventually tend to lessen the present uncertainty of his researches? or more desirable, than improvements in the various and complicated machinery by means of which he raises his ore from the bowels of the earth, and fits it for the purposes of Art?

"To the Naturalist, to the Botanist, to the admirer of Nature in her rudest forms, Cornwall presents a most interesting field; and among our barrows, hill-castles, and cromlechs, the Antiquary may find no slight traces of the Ancient Britons, who amongst the wild fastnesses of Cornwall and of Wales long retained their native freedom, and where the Druids practised the dreadful rites of their bloody superstition.

"The Painter will remember that Cornwall was the birth-place of an Opie; and may not some future Opie want but the fostering stimulus of such a Society as this to call his talents forth?"

Immediately after the rising of the Annual Meeting, the Secretary received a letter from T. Vivian, Esq. accompanying copies in alabaster of the most celebrated ancient and modern statues.

SURRY INSTITUTION, 1821.

The following Courses of Lectures will be delivered in the ensuing Season:

1. On Painting, by C. F. Pack, Esq.; to commence on Friday the 2d of November, at Seven o'clock in the Evening precisely, and to be continued on each succeeding Friday.

2. On the Elements of Chemical Science, by John Murray, Esq., F.L.S. M.W.S., &c.; to commence on Tuesday, the 6th of November, and to be continued on each succeeding Tuesday at the same hour.

3. On Music, by W. Crotch, Mus. D. Professor of Music in the University of Oxford; and,

4. On Natural Philosophy, by Charles Frederick Partington, Esq.; early in 1822.

ASIATIC SOCIETY.

At a Meeting of this Society held at Calcutta on the 17th of February, an Account of the Trigonometrical and Astronomical Operations for determining the heights and positions of the principal peaks of the Himalaya Mountains, situated between the latitudes of 31° 53′ 10″ and 30° 18′ 30″ north, and the longitudes of 77° 34′ 04″ and 79° 57′ 22″ east; by Captain J. A. Hodgson, 10

10th regt. N. I., and Lieutenant J. D. Herbert, 8th regt. N. I., was laid before the Society at this Meeting.

This paper is arranged under the following heads:

1. A general introductory account of the origin and progress of the Survey, of the nature of the country, of the instruments made use of, and of the modes of calculation.

2. Table of the latitudes of five principal Trigonometrical Stations observed with the reflecting circle and circular astronomical instrument; containing the results of 122 crossed observations of the sun and stars on both sides of the zenith, at the station near Seharunpore, in the plains of the Doab, and of 177 on the mountain station of the Chour, of 61 at the Fort of Bairaut, of 32 at Soorkunda, and of 29 at Wartoo, which three last stations are also on lofty mountains.

3. The longitude of the 1st meridian of the Survey, deduced from 21 immersions and emersions of Jupiter's first satellite, observed with Dollond's achromatic refracting telescope, of 42 inches distance, at the station near Seharunpore, or reduced to it.

4. A general account of the measurement of a base line of 217,48 feet in the Deyrah Doon, with explanations of the methods, instruments, and apparatus constructed for the purpose, and drawings of the same; and an account of the small and primary triangulation proceeding from the measured base to connect the stations of Seharunpore, the Choor, Biraut, Soorkunda and Budragh. And a table of the lines and angles of the 39 small triangles, arranged in columns under the following heads of data:

Angles observed at the three stations.—Angles reduced to the centre.—Angles for calculation.—Logarithmic lines.—Logarithms of the sides.—Length of the sides in feet.

5. A similar table of [illegible] largest triangles, showing the distances of other Trigonometrical Stations, and of snowy and other mountains and principal points.

6. Table exhibiting the heights above the sea of 38 snowy peaks, the columns containing the following data:

Names of stations.—Altitudes observed therefrom.—Arc of distances to the observed peak.—Corrected elevation.—Tangent of the same.—Distance in feet.—Logarithm.—Logarithmic distance in feet.—Difference of level in feet.—Height of the observed peak above the sea.

The highest of the snowy peaks within the limits of the Survey appears to be 25,589 feet, and the lowest 16,043 feet above the sea; and there are 20 peaks more elevated than Chimborazo, the most lofty summit of the Andes.

7. Paper supplementary to the last, showing how to deduce

satisfactory mean values of the heights of the stations of observation, with notices on the terrestrial refraction, founded on reciprocally observed elevations and depressions. This, where one of the stations is on the plains at the height of 853 feet above the sea, and the others observed from it are from 6,500 to 11,500 feet above it, appears on the mean to be 1-11 19 of the arc; but when the lower station is 7,000 feet above the sea and the higher about 14,000, the refraction is on the mean 1-16 81 of the arc. To which is added a note of the Azimuth of the principal stations.

8. Latitudes, Longitudes, and Elevations of the stations of observation, and of snowy and other remarkable mountains and principal places.

9. APPENDIX, containing geodesic Calculations and Investigations, with twelve tables for facilitating the calculations, within the limits of the Survey, and explanations of their uses.

10. Complete detail of the measurement of each portion of the base line.

11. Plan of the small triangles.

12. Plan of the great triangles, comprehending also the small triangulation.

The Meeting determined that this elaborate and valuable paper should be printed in the 15th volume of the Researches, the 14th volume being now nearly completed.

LXX. *Intelligence and Miscellaneous Articles.*

BUST OF DR. HUTTON.

A SUBSCRIPTION has been opened for a Bust of Charles Hutton, LL.D. F.R.S. &c. &c. to be executed in marble by Mr. Sebastian Gahagan.

This bust is intended as a mark of high respect and veneration for the character of Dr. Hutton, and as a tribute of gratitude for his important labours in the advancement and diffusion of mathematical learning during the long period of sixty years:—a period which will be memorable in the history of science, on account of his meritorious services both as an Author and Teacher.

As an Author, it is well known that his numerous publications have been uniformly held in the greatest estimation, and that even his earliest productions continue as standard works of increasing popularity in every country where the English language is understood. His persevering exertions also, as the conductor of scientific journals, during the above period, have had the most powerful effect in exciting emulation, increasing the number of able mathematicians, and thus greatly enlarging the boundaries of useful science.

As a Teacher, too, his labours have been singularly successful, especially as Professor of Mathematics for nearly forty years in the Royal Military Academy at Woolwich; an Institution which, by his judicious plans and unremitting care, he raised to the highest degree of celebrity and national importance. To his instructions, indeed, and his improvements in Military Science, his country is deeply indebted for the superiority and success of the British Artillery and Engineers, in every part of the world, for the last half century.

Such are the important objects to which Dr. Hutton has constantly devoted his valuable time and talents; and such are his well-founded claims to the gratitude and admiration of every lover of science,—claims which must ensure to him the lasting fame of having been one of the most efficient promoters of mathematical knowledge in any age or country; especially in improving and simplifying those sciences which are conducive to great public utility.

And here it must be gratifying to add, that this extraordinary man, though now in his eighty-fifth year, is still an ardent, and occasionally an active promoter of Science.

⁂ Subscriptions (which are limited to one pound each) are received by Dr. Andrew, of Addiscombe; Francis Baily, Esq. of Gray's Inn; Dr. Gregory, of Woolwich; Dr. Kelly, of Finsbury-square; Daniel Moore, Esq. of Lincoln's Inn-square; and by Edward Troughton, Esq. Fleet-street.

A Model of the intended Bust is already completed, and is considered a very accurate likeness. It may be seen at the Sculptor's premises, No. 87, King-street, Edgware road. Casts of the Bust, at two guineas each, will be prepared for such friends of Dr. Hutton as may choose to order them: but the Marble Bust is to be given to the Doctor himself, with the hope that he will hereafter present it to some Scientific Institution.

NEW SHOWER BATH.

To Dr. Tilloch.

Sir,—I had been long convinced of the danger to be apprehended from the use of the common shower bath in particular diseases incident to humanity or delicate constitutions. The sudden death of Mr. Spratt, at Brighton, from the shower bath, is a verification.

Impressed with this conviction, I described an invention wherein the water was suspended by the resistance and upward pressure of the atmosphere, the fall of the water being regulated by a valve and lever. This machine intermits, and the whole is under the complete controul of the patient.

The bulk of the descending spherulæ of water is proportional to the orifices in the base, and the period of duration correspondent with that of the suspension of the valve.

It is also obvious, that if the recipient be supplied with water at 98° F., and the apertures are sufficiently minute, a heated dew will precipitate, and all the effects of the vapour bath be obtained.

Of this machine a description and figure have already appeared in No. IX. of The Edinburgh Philosophical Journal, and baths erected on this principle. But as some little difficulty may be experienced in preserving the horizontality of the bath (when supplied with water by the cistern) in its elevation to the required altitude, I beg to observe that I have reversed the arrangement. The *vessel with its valve, remains fixed and stationary* in its place, and the *cistern of supply is raised* by the winch to feed the bath; and thereafter lowered into its place to act as a platform for the patient. A shower bath on this last modified construction has already been erected at Derby, with the most satisfactory results. I am, &c.

Oct. 8, 1821. J. MURRAY.

METHOD FOR PRESERVING FLAME UNDER WATER.

[From the *Acts of Leipsic.*]

A Doctor of our University having lately proposed to furnish fishermen and divers with a method of preserving flame under water, contrived the fitting of, to a glass vessel, which shut very exactly, two pipes of leather; whereof one continually supplied the lower part of the vessel with fresh air, by means of a bellows with a sucker and single or double wind; and the other, that opened into the upper part of the vessel, and was long enough to be always above the surface of the water, served to give vent to the fuliginous vapours drawn by the current of air from the first pipe.—*Universal Mag., Nov.* 1761, p. 259.

ON THE USE OF FINE WIRE WORK, OR METALLIC GAUZE, AS A SUBSTITUTE FOR HORN, &c. BY ALEXIS ROCHON.

[From the *Journal de Physique.*]

"The great brittleness of glass was a sufficient obstacle to the use of that substance, in the place of horn, on account of the danger that would attend the breaking of such a lantern in the powder room, or in any other part of the ship where powder or other combustible matter might happen to be." Page 207.

"It suddenly occurred to me that I might fulfil the purpose I had in view, by a process which was entirely new, and could be more speedily completed. This led me to suppose that the wire-gauze which has been long used in England, for making sieves, might fulfil to a certain point the ends I had in view, provided it were coated with a substance that was transparent and impervious to the air." Page 209.

It is then recommended, when tinned and painted, to be plunged into a tub filled with melted glue, very pure and transparent, the heat of which should not be great, nor the consistency too thick. Then (page 211) to be defended from moisture by drying linseed oil, and at page 211 isinglass is stated to be preferable to glue.

"In trying to discover a fit varnish to protect my new kind of lantern from moisture, I did not make use of copal or any other resin, as the varnish made with them is always more or less brittle, but employed a perfect solution of the elastic gum in drying linseed oil." Page 123.—*The Repertory of Arts and Manufactures*, vol. x. First Series. London 1799. 8vo.

ON A NEW SALIFIABLE BASE DISCOVERED BY DR. G. BRUGNATELLI.

[From the *Giornale de Fisica*, t. iii, p. 464.]

The new substance is produced by the action of liquid acids on uric acid. Those that have been used are the sulphuric, nitric, muriatic, and acetic; and the uric acid may be either that of calculi or of birds or snakes. It is formed by adding concentrated sulphuric acid, for instance, in small quantities at a time, to uric acid, until a thick paste is formed; it will occasion swelling, the liberation of gas, and a particular odour. When these signs have ceased, add water, the mass will become very white; and, on standing, will separate into two parts. The solid portion is a neutral combination of the new base with sulphuric acid. The fluid is a portion of this compound dissolved in the excess of acid, and containing impurities. The sulphate is but little soluble in water; but the solution, decomposed by alkaline subcarbonates, yields a white light flocculent substance, which is the base in question. Muriatic acid is, perhaps, better than the sulphuric for the preparation of this substance, inasmuch as the muriate is more soluble. Acetic acid requires boiling to form it, and nitric acid produces it among other products at the time of its violent action.

The flocculent matter collected on a filter, appears like gelatine; in drying it contracts and splits, and when pulverized has the appearance of an earth. It has no taste or smell. It is slightly soluble in water, alcohol, acids, and alkalies. The impure acid solution is eminently distinguished by its property of giving a very fine azure precipitate with triple prussiate of potassa, and which may readily be distinguished, after a few experiments, from that caused by iron. It may, perhaps, be applicable to dyeing or painting. The neutral combination of the substance with acids does not give the blue precipitate, it requires for this purpose excess of acid.

This substance combines with various simple bodies. With iodine it forms a compound, at common temperatures, of a dull yellow colour, resolved by heat into its two principles. When fused with sulphur they unite together; its compound with phosphorus is of a fine red colour, and, when dissolved in water, occasions the formation of phosphuretted hydrogen, and a phosphate.

This substance has extraordinary powers of resisting heat. It might be taken for an earth, or metallic oxide, in this respect. The following are given as experimental demonstrations of its properties:—An acid solution, put on a plate of zinc, gave a yellow spot with metallic splendour. This, well washed, dissolved in an acid, and tested by triple prussiate of potassa, gave a blue-white precipitate; the blue colour being attributed to the new substance. The solution that had acted on the zinc gave no blue colour with the test, but only a white.

A portion of it mixed with lamp-black and oil, and heated violently in a crucible for half an hour, left a reddish crust, the solution of which, in acids, gave an azure precipitate with the triple prussiate.

The azure matter burned in the fire with facility, and left a residuum of a bright red colour, if the heat had been intense; but if moderate and continued, the residuum is scarcely red, and when placed in water produces flocculi of the substance and bubbles of the gas.

Ammonia dissolves the substance, making it first yellow, then green; when heated moderately, a residuum is obtained of a yellow metallic colour; if more heated it becomes white, and does not seem to differ from the substance first dissolved. The yellow matter dissolved in dilute acid gives a red tint to ferroprussiate of potassa, which exposed to the air becomes green. Other changes take place.

Nitric acid appears to alter the nature of the new substance. When it is added in a concentrated state to the substance, or its salts, the prussiate does not then produce a blue precipitate, but a yellow tinge. Sulphuric acid, when assisted by heat, offers similar phænomena.

One cannot help suspecting, that the blue precipitates in these experiments are occasioned by iron, and yet it is difficult to conceive how that metal, if present, should escape the observation of Dr. Brugnatelli. Further experiments are required, and of a more decisive nature, to clear up this matter.

PRIZE QUESTIONS.

The Society of Apothecaries of Paris have offered a prize of 600 francs for—1. The best determination in what manner charcoal acts in discoloration, and what are the changes it undergoes during the action.—2. What is the influence exercised during the operation by any foreign substances which the charcoal may contain.—And, 3. To establish whether the texture of animal charcoal is not one of the essential causes of its more marked action on colouring substances. A prize of 300 francs will also be given for the best vegetable analysis, such analysis to be made on a substance used in medicine, or in the arts. The time is limited to April 1, 1822.

CONTAMINATION OF SALT FOR MANUFACTORIES.

The following question having been proposed to the Academy of Sciences by the French Ministry: "What are the processes to be adopted in contaminating common salt without injury to the soda manufactories, which will not permit of its re-appropriation to the uses of common life by any secret process, or at so little expense as to make the chances or the profits encourage fraud?"

The Academy in answering say, That it is impossible to resolve the question because of the high price of salt, but that the following means will render the fraud the most difficult.

1. Colour the salt by $\frac{1}{400}$ of wood charcoal.
2. Infect it by $\frac{1}{200}$ of oil distilled from animal substances, or by $\frac{1}{100}$ of tar.
3. To make the mixture in the magazines.

SHOWER OF SNAILS.

A Bristol Paper says—"The inhabitants of this city have lately been amused with the exhibition and sale in our streets of a collection of snail-shells, which are reported to have fallen, or we should more accurately say, made their sudden appearance in a field of about three acres, belonging to a farmer at Tockington. 'An Observer of Nature' has obligingly directed our attention to the natural history of this snail in Montagu's *Testacea Britannica.* Its name is *Limax virgata,* or Zoned Snail Shell.

'It may be considered,' he says, 'as a local species; but is found in prodigious abundance in some sandy or barren stony situations, most plentifully near the coast, especially about Whitsand Bay, Cornwall, and in the south of Devonshire, where it is believed they contribute not a little to fatten the sheep, the ground being covered with them.' This snail occurs also abundantly in the neighbourhood of Bristol and county of Somerset. We witnessed ourselves in a field belonging to Capt. Parish, at Timsbury, a few years since, an innumerable accumulation of them. On approaching heat they are observed to leave their hiding-place near the roots of grass, crawling upon the leaves and plants near it, and thus become visible to the superficial observer. From this remark of Montagu, and the well-known fact that snails furnish much nourishing matter, it would be perhaps best for the farmer belonging to the field at Tockington to turn into it a flock of sheep, which would soon crush the snails in eating them with the grass, and would doubtless improve thereby. In this phenomenon, the philosophic mind will easily trace the provision of Nature to render these snails (fattened near the roots of the succulent grass) a pasture, when parched by the rays of the sun, of a most nourishing nature to herbaceous animals. Common rumour says, 'that the snails fell in a great shower, which continued upwards of an hour, and that the earth's surface was covered, nearly six acres, three inches deep!!"

The Gloucester Herald says—"When we first heard the report of a shower of snails having fallen on Thursday week, near Tockington, in this county, we must confess we suspected the tale to be intended as the test of our credulity; but the fact has been subsequently authenticated by so many respectable persons, and having seen from different sources so considerable a number of those little curled light-coloured shells, with a streak of brown, and containing a living fish inside, we feel confident of the truth of the assertion. They fell like a shower of hail, and covered nearly an inch deep, a surface of about three acres, and great numbers were distributed to a much greater extent; shortly after this a storm swept so large a quantity into an adjoining ditch, that they were taken up in shovels-full, and travellers were furnished with what quantity they chose to take, and they were soon carried into the principal towns of this and the surrounding counties!!!"

MAD DOGS.

In the Medical and Physical Journal, a correspondent states, that it has been noticed that the *rabies canina* affects male dogs invariably, and never female.

TANNING.

A tanner, near Portsmouth, has lately discovered a most important process, whereby crop hides are tanned in *four months*, and made to *overweigh* the raw halves (which is the present common standard) from five to ten pounds upon each hide: consequently the advantages are double and triple—first, in point of saving so much *time*; secondly, in point of *weight*; and, thirdly, because *returns* are made *thrice* a year instead of *once*, according to the present practice of the trade.

EX-KING OF SWEDEN.

Colonel Gustavson, the Ex-King of Sweden, has for some time past applied himself to philosophical studies. He has just published a work at Francfort, but not for sale; it is distributed gratis, by the illustrious author, to the *amateurs* of arts and sciences. It is written in the French language, and is dedicated to the Royal Academy of Arts at Norway. It is entitled "Reflections upon the Phænomenon the *Aurora Borealis*, and its relation with the Diurnal Movement." The journals of Hamburg announce the arrival of several copies of the work at Stockholm, where they are now translating it into the Swedish language.—*Gazette de France.*

OBITUARY.

The useful Arts, aided by the ingenious applications of Science, have sustained a heavy loss, in the person of Mr. *Robert Salmon*, who for more than 30 years past has resided in the Park of the Dukes of Bedford, at Woburn, and conducted the architectural and mechanical departments of that extensive Establishment, and who since the late Duke's decease, and the retirement of Mr. Farey, has also conducted the *pruning*, thinning and management of the very extensive Plantations and Woods of His Grace.

Mr. Salmon has been the inventor of a considerable number of useful machines and implements for which patents have been granted, and which will be found recorded in the volumes of the "Repertory of Arts:" numerous others of his inventions were presented to the Society of Arts in the Adelphi, and by them liberally rewarded, and published in their annual volumes of "Transactions:" besides which, several well-deserved honorary marks of distinction were bestowed on Mr. S.'s ingenious inventions, at the Woburn Sheep-shearings. Mr. Salmon was born in 1763, and died on the 6th of October 1821: a surviving Brother and Sister, and nearly all the Servants in the extensive Establishment to which Mr. Salmon belonged, sorrowfully followed his Remains to the place of their interment, in Woburn church-yard.

THE LATE MR. CUSAC.

[From a Correspondent.]

We gave some account in a former number of Mr. Cusac's researches into the ancient state of Britain and Ireland before the Christian æra, and previous to the entrance of Alexander the Great into Babylon, in which Tacitus and the Norwegian and Icelandic annalists appear as his chief guides. We also find he has left some dramatic pieces founded on the great events of that remote period, and which may ultimately once more replace our three Kingdoms on their ancient pinnacle of glory. With such a view they were probably composed.

> "In farthest Britain Romans yet may rise."—THOMSON.

He has also left a poem a good deal in the strain of Petrarch, which will shortly make its appearance.

In his papers on comets he supposes them to be globes of water; that, on return to perihelion, the solar rays (after sunset) strike on the mass of water, enter converging to the centre, where, after decussation, they emerge from the liquid globe diverging, and form the phenomenon in the Heavens called the comet's tail.

As to the use of these watery bodies—he thinks they were formed by nature to assist in giving a due temperature to our system.

LIST OF PATENTS FOR NEW INVENTIONS.

To Sir William Congreve, baronet, for certain improvements on his former patent bearing date the 19th day of October 1818, for certain new methods of constructing steam-engines.—Dated 28th September 1821.—6 months allowed to enrol specification.

To James Fergusson, of Newman-street, Oxford-street, stereotyper and printer, for improvements upon additions to and substitutes for certain materials or apparatus made use of in the process of printing from stereotype plates.—18th Oct.—2 mo.

To Stephen Hawkins, of the Strand, civil engineer, for certain improvements on air traps for privies, water-closets, close-stools, chamber conveniencies to which the same may be applicable.—18th Oct.—6 months.

To Thomas Lees the younger, of Birmingham, snuffer manufacturer, for certain improvements in the construction of snuffers.—18th Oct.—2 months.

To Peter Davey, of Old Swan Wharf, Chelsea, coal-merchant, for an improved preparation of coal for fuel.—18th Oct.—4 mo.

To John Poole, of Sheffield, for certain improvements in plaiting iron or steel with brass or copper, or copper alloyed with other metal or metals, both plain and ornamental, for the purpose of rolling and working into plates, sheets or bars, and such goods or wares to which the same may be found applicable.—18th Oct.—6 months.

To John Christophers, of New Broad-street, London, merchant, for certain improvements on or a substitute or substitutes for anchors.—18th Oct.—6 months.

To Owen Griffith, of Tryfan, in the county of Carnarvon, gentleman, for an improvement in the principle and construction of manufacturing or making of trusses for the cure of rupture or hernia, in whatever part or parts of the body it may be situated.—18th Oct.—6 months.

ATMOSPHERIC PHÆNOMENON.

[Extract from a private letter.]

Letterkenny, August 31, 1821.

The phenomena apparent here last Friday were the most awful and extraordinary ever witnessed by the oldest inhabitant of the vicinity. I have not met with any individual who could say that he had seen, read, or heard of, such an appearance in any latitude. About eleven A. M. there was a weak breeze from the south-west, the barometer at 'changeable,' with an appearance of heavy rain, which began to fall about forty minutes after eleven, and continued until twelve, at which time there was a dead calm, and the rain ceased entirely. The sun had not shone out during the morning, but a few minutes after twelve the darkness began to increase in the most extraordinary manner; at one there was not light sufficient to transact business in the common offices and shops; merchants were obliged to light candles; the dismay and terror of the lower classes of people were excessive in the extreme, every one crying out that the last day was come; all the domestic fowl went to roost; not a wild bird to be seen, or a twitter heard; mechanics and labourers quitted their work, exclaiming 'The day of judgment is arrived.' At this moment neither barometer nor thermometer was a line changed from what they had been at ten o'clock. There was a dead calm. The chimney smokes shot up in perpendicular columns, till lost in masses of dark clouds, with which the concave surface of the heavens was entirely covered. The appearance of those clouds was most astonishingly awful—they were something like those dark blue volumes of smoke which arise from an explosion of gunpowder, and piled on each other, tier above tier, from the horizon to the zenith, where they concentrated, so as to form the vertex of a Gothic arch.—Through small interstices, where those gigantic masses appeared to lap over each other, appeared to issue a faint gleam of sulphureous light.

At this moment, about one o'clock, the appearance of objects was wonderfully changed. Meadows of a light green, appeared dark green—objects of a dark green seemed quite a dark bottle-green, and the dark gravel of some roads appeared of a blackish blood colour. Men's faces and dresses were all changed in the same manner, so that people looked at each other with astonishment and awe. The colours were all of the finest tint and shade, very rich and mellow. I could not conceive any thing like them, except the brightest prismatic colours viewed through a glass slightly smoked, so as to give them a mellowness without destroying their brilliancy. On the surface of all objects there was observed a gentle undulatory motion, which had a very surprising effect. This I found was occasioned by the clouds, which, though they seemed to the naked eye perfectly still, yet, when viewed through a telescope, appeared to oscillate something after the manner of the *aurora borealis*, without changing their relative positions.

This darkness continued until two o'clock, and to such a degree as that scarcely any person could read or write within doors without approaching close to the windows.

A little after two, there was observed a gentle motion of the clouds from the south-west; they moved almost impereptibly to the north and east, and about three the darkness was dispelled; cocks began to crow, and swallows to fly about, as if it had been early in the morning.

I have not yet learned how far this darkness extended.—I am, &c. E. F.

BAROMETRIC OBSERVATIONS.

To Dr. Tilloch.

Howland-street, Oct. 12, 1821.

Sir,—Of the four monthly sets of Dr. W. Burney's Barometric Observations, which he mentions in p. 238, as having been sent to me, the two last have already been correctly printed in pages 155 and 237: the other two sets are as follows, viz.

Barometrical Observations at *Gosport*, June 11th, 1821, under all the circumstances of Thermometer, Hygrometer, Winds and Clouds, which are mentioned in p. 75.

Hours	8	9	10	11	12
Inches	29·97	29·99	30·01	30·01	30 04.

Barometrical Observations at *Gosport*, July 9th, as above, p. 76.

Hours	8	9	10	11	12
Inches	30·24	30·25	30·26	30·26	30·25.

I am your obedient servant,
John Farey Sen.

Observations by Dr. Burney, at Gosport; the height of his Barometer being 50 feet above low-water mark.

Hour.	Barom.	Ther. att.	Ther. det.	Hygr.	Wind	State of the Weather.
1821. A.M.	Inches	°	°	°		
Oct. 8th. 8h	30 08	58	58	94	S.W.	Steady rain and a brisk wind, at a quarter past 8 o'clock it ceased to rain, and the edge of the extensive *nimbus* was observed springing up from the western horizon.
9	30 08	59	59	88	W.	Faint sunshine through passing beds of *cirrus* and *cirrostratus*, in which was a trace of a solar *halo*, with a brisk westerly wind.
10	30 09	61	60	85	W.	Sunshine, with the same modifications of clouds as at 9 o'clock; also lofty *cirrocumuli*, and nascent *cumuli* floating beneath them towards the east.
11	30·14	63	62	76	N.W.byW	Sunshine and a brisk wind with bright *cumuli* and *cumulostrati* beneath *cirrus*.
12	30·15	64	62	78	W.N.W.	Do. do., but the air more moist.
P.M 1	30·17	64	63	67	N.W.	Do. do, the clouds at a greater altitude, and the index of the hygrometer advancing towards dryness—the barometer continued to rise in the afternoon, the wind having veered to N W.

Arundel, Oct. 10, 1821.

Sir,—The following Barometrical Observations were made at this place on the 10th of September, and the 8th instant.

I am, sir, your obedient servant,

G. Constable.

Hour.	Barom	Thermo. att.	Thermo. det.	Wind.	Weather.
Sept. 10th,	°	°	°		
8h	29·685	61 0	60·5	W. moder.	Heavy rain.
9	29·700	61 0	60·5	W. calm.	Cloudy.
10	29 705	61 5	61·0	W. by N. do.	Rain.
11	29 705	62·0	61·0	W. by S. brisk.	Do.
12	29·705	62 0	61 0	W. calm.	Cloudy.
P.M. 1	29·710	63·0	61·5	W do.	Do.
Oct. 8th,					
8	29 950	60 5	60 0	S.W. brisk.	Rain.
9	29 955	61·0	60 0	S. by W. moder.	Do.
10	29 980	60·5	59·5	W. calm.	Gleams of sunshine.
11	29 988	60 5	60 0	W. do.	Sunshine with clou.
12	29·995	61 5	60 5	W. do.	Do.
P.M. 1	30·000	62·5	61 0	W. do.	Do.

Crumpsall, L[illegible]shire, Oct. 9, 1821.

Sir,—I send you observations made here, and at Manchester, on the 10th of September, and the 8th of October.

Your obedient servant,

To Dr. Tilloch. John Blackwall.

CRUMPSALL.

1821 A.M.	Bar.	Ther. att.	Ther. det.	Wind.		Weather.
Sept. 10th 8h.	29 295	56°	54° 5	S.	light.	Cloudy, with gleams of sunsh.
9	29.310	57 5	57	S.W.	fresh.	Do.
10	29.310	59	59·5	S.W.	do.	Do.
11	29.320	60 5	61	S.W.	do.	Do.
12	29.320	61·5	62	S.W.	do.	Do.
P.M. 1	29·330	62	63 5	S.W.	do.	Do.
Oct. 8th, A.M.						
8	29 520	56	54 5	S.W.	do.	Foggy, with faint sunshine.
9	29·520	56	55·7	S W.	do.	Cloudy, with gleams of sunsh.
10	29·510	57	56·5	S W.	do.	Do.
11	29 560	56	55	S.W	do.	Do.
12	29 575	56	56 2	W.	brisk.	Do.
P M. 1	29·600	55·7	55	W.	high.	Cloudy.

MANCHESTER.

1821. A.M.	Bar.	Ther. att.	Ther. det.	Wind.		Weather.
Sept. 10th, 8h.	29 500	62°	60° 5	S.W.	fresh.	Cloudy.
9	29 530	64	63·5	S W.	do.	do.
10	29 540	66	65·5	S W.	brisk.	do.
11	29.545	66·5	66	S.W.	do.	Cloudy, with sun gleams
12	29.560	69	67·5	S.W.	fresh.	Cloudy.
P M. 1	29·570		67 5	S.W.	do.	Cloudy, with sun glea
Oct. 8th, A.M.						
8	29·700	61	54	S.W.	do.	Clear, and sunny.
9	29·720	62 5	56	S W.	do.	do.
10	29·750	63	58 5	S.W.	do.	Cloudy.
11	29·755	61	59	S.W.	do.	Clear, and sunny.
12	29·780	65	59	S.W.	do.	Fine, with some *cirri.*
P.M. 1	29·810	68	59·5	S.W.	brisk.	Cloudy.

Leighton, Oct. 24, 1821.

Dear Sir,—I beg leave to send you the observations made at Leighton on the 8th instant, with those of Colonel Beaufoy at Bushey.

LEIGH-

LEIGHTON.

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·704	55½	53	W.	small.	Rain.
9	29·712	55½	54	S.	do.	Cloudy.
10	29·736	56½	56	S.W.	do.	Fair.
11	29·748	57½	57	W.S.W.	do.	Fine.
12	29·762	58	60	W.	do.	Do.
1	29·771	58	59	W.	do.	Do.

A thermometer suspended at the middle of the barometrical tube averaged 3½° higher than the thermometer in the basin.

BUSHRY.

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·479	55·3	53	S.S.W.	fresh.	Rain.
9	29·493	55·3	53	W.N.W.	moder.	Do.
10	29·499	55·3	54·5	W.	do.	Cloudy.
11	29·512	55·3	56	W.	fresh.	Fine.
12	29·529	56·7	58	W.	do.	Do.
1	29·539	57.7	59	W.	do.	Do.

Calculated height of Bushey above Leighton, from the preceding month, by Colonel Beaufoy =220 9 feet.

I have also the pleasure to send you the observations made by Mr. Comfield, at Northampton, on the 13th of August last; and as the height of the lecture-room in which Mr. Comfield's instrument was placed, has been pretty accurately determined relative to the height of my instrument, the observations may be of use.

NORTHAMPTON.

1821.	Hour.	Barom.	Ther. att.	Ther. det.	Wind and Weather.
Aug. 13th.	8h 20m	29·821	61	59¼	
	9 4	29·821	62	61	
	10 0	29·821	63	64	
	11 0	29·807	65¼	66	
	12 0	29·804	65	66	

I have not at present been able to find any correct level of the River Thames, so as to fix the zero marks in London: until this is done, no good *general* table of heights can be properly published.

Yours truly,
B. BEVAN.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Sept. 15	20	68·	29·90	Cloudy
16	21	66·	29·80	Ditto—rain A.M.
17	22	61·5	29·70	Rain
18	23	68·	29·45	Stormy
19	24	63·5	29·48	Cloudy
20	25	54·5	29·60	Rain
21	26	66·5	29·34	Cloudy—rain A.M.
22	27	64·5	29·50	Fine
23	28	64·5	29·34	Cloudy—rain A.M.
24	29	56·5	29·35	Rain
25	30	60·5	29·65	Cloudy
26	new	67·5	29·55	Ditto
27	1	61·	29·48	Rain—thunder storm in afternoon.
28	2	63·	29·58	Cloudy—stormy with heavy rain at night.
29	3	56·5	29·10	Stormy
30	4	55·	29·50	Ditto
Oct. 1	5	60·5	29·25	Ditto
2	6	60·	29·79	Fine
3	7	66·5	29·48	Ditto
4	8	62·	29·20	Rain
5	9	53·	29·60	Fine
6	10	61·	29·70	Cloudy
7	11	64·	29·70	Fine—brisk wind.
8	12	60·	29·65	Cloudy—rain in the morning.
9	full	57·	30·	Fine
10	14	59·	29·80	Ditto
11	15	59·5	29·45	Ditto
12	16	56·	29·65	Ditto
13	17	54·5	30·12	Ditto
14	18	57·	30·12	Ditto

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Sept. 27	56	65	53	29·94	Showery
28	52	65	56	·88	Fair with high wind.
29	56	59	49	·57	Cloudy with do.
30	47	59	59	·92	Fair
Oct. 1	60	65	52	·74	Fair
2	52	59	56	30·20	Fair
3	60	65	62	29·90	Showery
4	62	62	60	·60	Rain
5	47	58	47	·99	Fair
6	50	64	52	30·09	Fair
7	56	65	57	·08	Fair
8	57	61	50	·05	Showery
9	45	59	49	·35	Fair
10	47	61	50	·09	Fair
11	49	55	49	29·74	Cloudy
12	50	57	49	·95	Fair
13	46	58	48	30·39	Fair
14	44	59	50	.39	Fair
15	46	52	44	·31	Showery
16	42	52	48	·21	Fair
17	46	52	50	·18	Fair
18	50	59	50	29·98	Cloudy
19	51	53	50	·86	Cloudy
20	51	53	43	·02	Stormy
21	41	53	46	·20	Fair
22	45	54	45	·27	Fair
23	49	52	48	·29	Rain
24	46	52	44	·54	Cloudy
25	46	53	51	30·00	Cloudy
26	51	59	54	·11	Cloudy

N B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the

8th Oct. 8 o'Clock M. Barom. 29 994 Ther. attached 63° Detached 57
— — 9 — — — — ·996 — — — 63 — — 57
— — 11 — — — — 30·014 — — — 63 — — 58
— — 1 — N. — — ·050 — — — 62 — — 61

LXXI. *Observations on the present State of Nautical Astronomy; with Remarks on the Expediency of promoting a more general Acquaintance with the modern Improvements in the Science among the Seamen in the British Merchant Service.* *By* EDWARD RIDDLE, *late Master of the Trinity-House School, Newcastle; now Master of the Upper School, Royal Naval Asylum, Greenwich.*

IF there be any circumstance by which the present age is pre-eminently distinguished, it is the advantage with which the results of scientific inquiry have been applied in the practical concerns of life. This observation is applicable to almost every branch of philosophical investigation; but its truth is strikingly obvious with respect to the science of Astronomy. For though this science has been cultivated from the earliest times, it is not more than a century and a half since our countryman Newton first discovered its physical theory; and it is to the age in which we live that the honour belongs, not only of perfecting the science, but of bringing its most important practical applications within the reach of every one whom they are likely to benefit.

We should mistake, however, if we imagined that the labours of all who had previously cultivated this science must have been utterly thrown away. The diligence of observers had collected a mass of facts, which, long before the time of Newton, had given several sagacious individuals a pretty accurate conception of the proximate cause of the leading planetary phenomena; and which also furnished such data for the application of the Newtonian theory, such tests of its truth, and such materials for improving it, as modern observations could not in themselves have supplied.

Now, though little confidence can be placed in the individual accuracy of very ancient observations, they derive a real and important value from a circumstance which gives to many useless objects an *imaginary one;* namely, their *antiquity*. Suppose, for example, an astronomer at the beginning of the Christian æra should have erred six hours in stating the time at which he observed a total eclipse of the sun. If he were quite certain that during a period of the eclipse the darkness was total, his mistake respecting the time at which it happened, monstrous as it is in itself, would shrink into insignificance if his observation were compared with the well determined time of a corresponding modern eclipse, to discover the length of a mean lunation; as the resulting error would scarcely exceed a single second of time.

It is in this view chiefly that old astronomical observations are valuable; for, down to a comparatively late period, when they are compared with each other, they are as discordant as the state of the science, and the rude kind of instruments employed in observing, might lead us to expect.

But from the moment that the principle of gravitation was discovered, and applied to account for the anomalies of the planetary motions, it is curious to observe how steadily and how rapidly the science has advanced towards perfection. It resulted from this principle, That each planet in the system exerted a disturbing force on the motions of the others, and that the force of each depended both on its distance and its quantity of matter, or its *weight*. To determine the distances of the planets, might appear a task sufficiently difficult; but to estimate with any precision the comparative *weights* of those bodies, would seem an undertaking which the limited faculties of man could scarcely be expected to accomplish. But, supposing the principle of gravitation univerally diffused, and the law of its operation invariable, it was easy to perceive, that on the correct determination of the distances and the masses of the planets, the ultimate perfection of the astronomy of the solar system entirely depended. It was also very obvious, that their comparative masses could only be discovered by observing the comparative magnitude of the effects which they produced on the motions of each other; and that though refined observations might show, at any time, the ultimate derangement in the motion of any planet produced by the action of *all* the others; yet to disentangle the effect of one from this observed effect of the whole, and to assign to each that portion which was due to its weight in the system, was a task which required no ordinary attainments in science, and no small share of patience and sagacity.

Before this, however, could be attempted with any prospect of success, the most accurate observations were necessary on the places of the fixed stars, and also on those of the planets in different parts of their orbits, and in a variety of situations with respect to each other. To make such observations with requisite correctness, instruments of greater delicacy were required than any that had before been employed; and for fixed observatories such instruments were not long wanting.

The positions of the principal fixed stars were then determined with such precision, and the phænomena of the planetary motions were so accurately observed, that the effect which the attraction of the planets produced on the motions of each other was found in many cases to be distinctly perceptible. And repeated observations have at length furnished data, from which the masses of the larger planets have been computed with an exactness that appears nearly sufficient for all practical purposes.

From this refinement in the art of observing, some curious, important, and previously unnoticed consequences, both of the laws of motion and the laws of gravitation, were first discovered *practically*; viz. the *nutation of the earth's axis*, and the *aberration of light*. The former of these is a small periodical motion of the earth's axis produced by the action of the moon. The latter is a small annual change in the apparent places of fixed stars, resulting from the earth's revolution in its orbit combined with the progressive motion of light.

It was known from theoretical considerations, that if the distance of any one of the planets could be determined, the distances of all the others could be found from observations on their times of revolution. But the only method of finding the distance of the sun from the earth, that appeared likely to give results on which any confidence could be placed, was an indirect one depending on the duration of the transit of Venus as observed in different parts of the earth. And it was chiefly to observe a transit of this planet that Captain Cook's first voyage to the South Sea was undertaken. This leading object of the voyage was accomplished as satisfactorily as all the subordinate ones were; and from the observations made on that occasion, and the only other occasion of the kind that has happened during the last century, the distances of the planets from the sun and from each other have been determined with a precision which, from the nature of the problem, there is little reason to hope will ever be greatly exceeded.

Not satisfied with simply comparing the masses of the planets with each other, philosophers have endeavoured to devise means of comparing their mean densities with that of some known terrestrial substance. In this most curious and elaborate research, the distinguished talents of Dr. Maskelyne and Dr. Hutton have been advantageously conjoined; and the result of their inquiries with respect to the density of the earth, appears entitled to all the confidence that can be placed in conclusions deduced from operations of so very delicate a nature.

While the art of observation has been thus prolific in results, the intricate and difficult researches of those who have undertaken to trace the principle of gravitation into all its consequences, have produced discoveries that can scarcely be considered as less curious or important. They have shown that, notwithstanding the continual changes that take place in the planetary motions, notwithstanding the disturbing force which they are continually exerting on each other, their distances from the sun, the species of their orbits, and the mutual inclination of their planes, are subject only to periodical changes, and return at regular though distant periods to the same state; that amidst the ever varying phænomena which their configurations present, the law that connects them with each other ensures the stability of the whole system. And, what is of still greater importance, with the aid of data obtained from observation, they have furnished us with formulas,

mulas, from which we can compute with almost perfect exactness where any planet in the system will be found at any given instant; and they have thus enabled us to apply to important practical purposes the result of researches the most arduous and difficult that have ever engaged the attention of mankind.

It was the proud boast of a distinguished philosopher, that at the close of the eighteenth century there remained not an observed phænomenon of the planetary motions that had not been completely accounted for.

Among those who have contributed to this glorious achievement, it is satisfactory to observe that the names of our countrymen hold a distinguished place. It is too much to expect that men worthy in every way to be considered as the successors of Newton will readily be found in any country; but the labours of that great man in the science of Astronomy have not been inadequately followed up by the intelligence and activity of those who have happily been selected to fill the situation of Astronomer Royal in this country; a succession of men of whom it is slight praise to say that they have been in every respect worthy of their office. Whilst we pride ourselves, however, on the aid which our admirable observers have afforded towards completing this great work, we must acknowledge that more than an equal share of the honour of extending and perfecting the *theory* of the science is due to the philosophers of a neighbouring country.

The great object of all the exertions that have been made to bring this science to perfection, has been the improvement of the art of navigation; and in that art the finding of the longitude by celestial observations has for ages been considered as the grand desideratum. Of all the methods that have been proposed to solve this problem, none has been found capable of being reduced to practice at sea, except that by observations on the distance of the moon from the sun or a fixed star. Accordingly, for the last century, the attention of astronomers to every thing calculated to bring this method to perfection has been unremitted. Every improvement in the instruments of observation, and every advance in the theory of astronomy, has contributed to increase the utility of the lunar method of finding the longitude; and it has at length been brought to a degree of perfection, which forty years ago those best acquainted with the subject could scarcely have anticipated.

When Mr. Flamsteed, the first Astronomer Royal, received his appointment in 1675, he was enjoined "to apply himself with the utmost care and diligence to rectify the tables of the motions of the heavens, and the places of the fixed stars, in order to find out the so much desired longitude at sea, for perfecting the art of navigation." But such, in his time, was the state of Astronomy, that in some cases the place of the moon in the heavens could not be computed to within less than 10 or 12 minutes of the truth;—an error which would sometimes have rendered the longitude deduced from a lunar distance, uncertain to the extent of eight degrees. Therefore, though this method of finding the longitude at sea appeared the most eligible of any that presented itself, it was at that time abandoned as altogether impracticable.

When Dr. Maskelyne succeeded to the situation in 1765, such advances had been made in astronomical science, that under his auspices this method of finding the longitude (which in the interval had never been lost sight of) was again brought forward; and after devoting a great portion of his long and valuable life to bring the method to perfection, he had the satisfaction of seeing his meritorious exertions crowned with almost complete success.

One of the great obstacles to its introduction into general practice, was the difficulty in making the necessary calculations from the tables and formulas which the labours of astronomers had supplied. But this formidable objection was completely obviated by the publication of the Nautical Almanac, which on Dr. Maskelyne's suggestion was undertaken by the Board of Longitude in 1766, and it has ever since been continued annually.

Before mariners in general, however, could be materially benefited by all these efforts for their advantage, it was requisite that they should be furnished with such instruments for observing, as they could use with readiness, and depend upon with confidence; and with *simple* as well as appropriate rules for making the necessary calculations after their observations were completed.

The artists of this country answered well to the call which this consideration made upon their talents, and devised such improvements in the construction and graduation of quadrants, sextants and circles, for nautical observations, that nothing appears left to be desired on the subject. And in all the useful problems in the practice of nautical astronomy, the methods of performing the calculations have been so simplified, that it is not easy to conceive for what situation connected with the navigation of a ship, the person is fitted who is incapable of comprehending them.

It has been fortunate for Astronomy, that theory and observation have advanced simultaneously. From successive improvements in the instruments and the art of observation, the abstruse deductions of theory have been regularly subjected to the sound practical test of comparison with observed phænomena; while the apparently anomalous facts of observation have pointed to important consequences of the theory, which might otherwise for a time have been either neglected or overlooked. Under these salutary checks, the progress of the science has been distinguished by a steadiness of which the history of no other science furnishes an example.

In all sciences, many of the greatest improvements have been effected by private individuals who have pursued them for their own gratification, and without any particular view towards personal advantage. But the cultivation of this science has been long and assiduously promoted by the governments of Europe, on account of its public importance.

Deeply sensible of the advantages which a maritime nation must derive from every improvement in navigation and maritime geography (which are entirely dependent for their perfection on Astronomy), the English Government has distinguished itself above all others by the munificence with which it has rewarded every discovery tending in any way either to facilitate the practice, or to advance the theory, of the science. And the recent order for the establishment of an observatory, on the most liberal scale, at the Cape of Good Hope, may be considered as a pledge that the zeal of Government for the promotion of this science has suffered no abatement. Indeed, in addition to the encouragement which it has so liberally afforded to those who have contributed to the improvement of the science, it has lately gone an important step further; and has even employed the weight of its *authority*, to assist in diffusing among nautical men a knowledge of those branches of it which are immediately connected with navigation; having made a practical knowledge of such branches an absolute *sine qua non* to promotion in the navy.

In the public spirit and liberality of the East India Company, also, every improvement in this science has met with the most prompt, decided, and efficient support; and the discernment which they have always evinced in promoting those only to the command of their ships whose professional merits give them the highest claim to advancement, has excited a spirit of emulation among their officers, which has been productive of the happiest effects.

But though in the general merchant service there are many estimable individuals whose professional accomplishments would do credit to any employment, it is deeply to be regretted that the number of such persons is not much greater than it is. Even among those intrusted with the navigation of ships to Archangel, Greenland, the West Indies, and every port on the eastern coast of North and South America, a great majority depend still altogether for their longitude on data obtained from the compass and the log. Indeed, celestial observations of any kind are so little practised, that "to take an observation" is a phrase, which, generally speaking, signifies to observe the sun's meridian altitude, and nothing more. The number is comparatively small of those who practise the method of finding the latitude by double altitudes of the sun; and altitudes of the moon or stars for that purpose are scarcely ever taken at all. The variation of the compass is generally taken from charts; and of those navigators employed in the leading branches of general commerce, who have qualified themselves to determine the longitude by celestial observations, the number is altogether trifling.

It happens, in consequence, that in voyages of considerable length the accumulated error in longitude amounts sometimes to several degrees. Ships bound from Greenland for the eastern coast of England or Scotland, often make the coast of Norway in the finest weather possible. When ships from Archangel reach the latitude of Shetland, it frequently becomes a grave question, Whether do these islands lie east or west? an interrogatory to which the reckoning from the log often makes a very equivocal response. But, supposing the latitude determined with the utmost accuracy, errors like these in the position of a ship at sea will frequently arise in spite of the greatest skill or experience, if the mariner is unable to determine his *longitude* also by independent observations. Mistakes in the steering, which defy detection, the effect of unknown currents, and numberless other circumstances on both the course and the distance, vitiate the data from which the place of the ship by the sea reckoning must be computed. It happens sometimes, indeed, that one mistake is corrected by another, and that, while every part of the reckoning is more or less erroneous, the result of the whole is nearly right. When this occurs, a ship is said to make *a good land fall.* But that it happens so at any time, is entirely a matter of chance.

Persons who have long been accustomed to the navigation of shallows and contracted seas, such as the North Sea, the Baltic, &c., generally acquire such a knowledge of the soundings as enables them by the lead alone to make a tolerable guess at their situation. *In these seas, it is not common to make any calculation for the longitude, even from the reckoning;* and the majority of masters engaged in navigating them hold the aids of science in utter scorn. And, indeed, if we were to judge only from the scanty attainments of most of those who, in such places, are intrusted with the charge of ships, we might consider the inutility of any particular knowledge of navigation as pretty generally admitted.

It is a very few years since the master of a ship bound from Shields for the Baltic in the early part of the spring, mistook the day of the month; and though all his observations for the latitude were of course at least twenty miles erroneous, he made the land in a very satisfactory manner. Even in favourable weather, it has sometimes happened that the master of a ship has looked for the Naze during the greater part of two days. Passing over the marvellous escape of the coaster, who, in a voyage from London to Shields, almost circumnavigated the island in search of his port, the experience of every person acquainted with maritime affairs will point out many instances in which ships have been preserved rather in *spite* of, than in consequence of, the plans of those who commanded them. But without at all undervaluing the importance of local information, it may be safely affirmed, that even in those quarters where it is almost exclusively depended on, many of the shipwrecks which happen might be prevented, if the mariner's acquaintance with the modern improvements in nautical science enabled him to avail himself of many opportunities which present themselves for determining his situation. If, after an intelligent seaman has been buffeted about for ten or twenty days with an adverse wind and in thick weather, the clouds should disperse and the sun and the moon should appear, he soon becomes as well assured of his situation as if he had just lost sight of a well known coast. How different is the situation of an ordinary mariner, who, after once losing confidence in his reckoning, must for the remainder of his voyage remain in doubt and uncertainty!

It is of importance in any emergency, that a man should clearly understand the nature of the difficulty with which he has to contend, that he may bring his resources into action with a knowledge of the effect which, if successful, they must produce. With what advantages does the mariner approach a situation of danger, who knows precisely where to look for it! But how in such circumstances must the man be affected who, knowing himself in peril, is in doubt what way to run! Of what importance to such men is the knowledge of every thing that may contribute to inform them of their true situation!

Before the discovery of any practicable method of finding the longitude at sea by observation, the mariner who estimated his longitude as carefully as he could from the common reckoning, did all that on the subject could be expected from him; all, indeed, that it was possible to do. But, however careful he might be in his estimation, or however judicious in making allowances for circumstances, the result would often be surprisingly erroneous, and the error would frequently lead to disastrous consequences. The chance of such mistakes, however, was then properly classed among the common sea risks, which, whatever were their magnitude, were quite unavoidable. But there would be no propriety in so classing them now, when, with very little trouble, any mariner of common capacity may qualify himself for determining his situation at sea by celestial observations, as frequently and correctly as in the practice of seamanship need be desired.

The ordinary dangers of the sea are sufficiently great of themselves, and ought not to be increased by the want of any attainable

able information in those to whom the business of navigation is intrusted. But he who undertakes the management of a ship, without having qualified himself in the best possible manner for the proper discharge of his duty, subjects the lives of his crew and the property of his employers to an *additional* risk, of which they may consider it providential if they feel not the consequences.

Another advantage arising from the mariner's being able to determine his *longitude* by observation, as well as his latitude, is, that, besides his being enabled to avoid dangers to which he might otherwise be exposed, and to navigate his vessel with greater confidence and comfort to himself, he will generally reach his port by a shorter route than a seaman of more primitive attainments. A person assured of his situation both in latitude and longitude will, if circumstances permit him, steer directly for the place at which he wishes to be; but he whose longitude depends solely on his sea reckoning must first, and at a *wary* distance from his port, reach the latitude in which it lies. Thus, besides the risk arising from the uncertainty of the reckoning, the voyage is unavoidably prolonged.

Among the causes which have prevented the seamen of the North of England from benefiting, generally, by the advantages which modern science has conferred on navigation, the chief one unquestionably is the nature of the employment in which by far the greater part of them are bred. In that great nursery of able seamen, the coal trade, a minute local knowledge of the coast, and dexterity in the management of a ship, comprehend almost the whole of the nautical information that is required, and ordinary men are little inclined to concern themselves about attainments which their immediate wants do not force on their attention.

When persons brought up in this employment are engaged in any other where higher attainments may be expected, they endeavour to make up by *watchfulness* what they want in science. As uncertain they must be, their care is to err on the side of safety; and the danger is not trifling from which their skill in seamanship will not extricate them.

Those who have laboured to introduce among such men a more general acquaintance with the modern improvements in nautical astronomy, have had to contend with that disinclination to change, and almost superstitious attachment to old methods, forms, and habits, for which seafaring men have always been remarkable. The success of their labours has been in some degree impeded also by the misguided efforts of some individuals, who have employed themselves in contriving and transforming certain empirical rules and methods for correcting the error of the reckoning in longitude, without any reference to observations made for that purpose; or as an adequate substitute for all such observations. Probably most of the authors of these rules are aware that they are founded entirely on conjectural principles, and that in applying them there is always a chance of increasing the error which they are intended to correct. Yet the attention of mariners is continually called to their republication under various forms; and without being accompanied by a single hint that any practicable method of finding the longitude at sea by observation has yet been discovered.

It has undoubtedly some relation to the peculiar views of such persons, that among many ordinary seamen an opinion prevails, that though the finding of the longitude by observation at sea is a matter of great and obvious utility, yet the methods which have been hitherto proposed for its determination are difficult to practise, and uncertain in their results.

On the difficulty of the subject it becomes not a man to give an opinion, who has never tried in earnest whether there is any thing in it beyond the reach of his own faculties. Indeed, any person acquainted with arithmetic, and who has no imperfection in his sight, may soon learn to practise the method of finding the longitude by lunar observations with perfect success. This remark is not made without due consideration; but it is too evident to admit of question, that one of the great causes which have prevented the general introduction of the lunar method of finding the longitude into general practice in the merchant service, is the little attention that has been paid to giving instructions *in the art of observing* by many teachers of navigation.

The certainty of the results will of course be proportioned to the care with which the necessary operations are performed; but in the hands of a careful observer, the greatest probable error is too insignificant to be regarded. That persons of indolent or slovenly habits are unable to derive much advantage from this, or any other method of finding the longitude by observation, is surely neither an argument against its utility, nor matter of surprise in itself.

To estimate fairly the weight of this objection, however, and to determine by actual experiment what confidence may now be placed in the results of lunar observations, the following series was taken to determine the longitude of the Trinity House School, Newcastle. The observations were made under a great variety of circumstances, and the uniformity of the results is certainly calculated to give confidence in the method by which they were obtained. The altitudes, which were *computed*, are omitted, for the sake of bringing the figures within the breadth of a page; but any person who thinks proper may re-calculate them, observing that the latitude of the place where the observations were made, is 54° 58½′ N.

Lunar Observations taken in 1821, to determine the Longitude of the Trinity House School, Newcastle.

Objects observed.	Day.	Ap. Time at Newcastle.	App. Dist.	True Dist.	Long. W.	Error in excess.	Error in defect.
		H. M. S.	° ′ ″	° ′ ″	° ′ ″	′ ″	′ ″
☉ and ☽	Feb. 5	23 27 45	50 52 18	50 47 19	1 32 15		5 2
... ...	... ...	23 51 42	51 4 0	51 0 50	1 31 0		6 17
☽ Ald.	... 6	6 59 36	55 40 35	55 2 57	1 31 45		5 32
☉ ... ☽	... 7	1 43 27	65 30 38	65 31 23	1 37 15		0 2
... ...	... ...	1 54 17	65 35 37	65 37 33	1 41 15	3 58	
... ...	... ...	23 49 23	77 54 6	77 59 58	1 25 0		12 17
... ...	... 8	1 39 16	78 48 8	78 39 49	1 31 30		5 47
... ...	... ...	2 17 1	79 5 1	79 0 25	1 34 15		3 2
☽ Pollux	... ...	6 24 10	69 50 4	69 25 34	1 41 15	3 58	
... ...	... ...	7 36 11	69 16 47	68 13 44	1 46 30	9 13	
☉ ... ☽	... ...	23 56 23	90 57 38	90 38 51	1 41 45	4 28	
... ...	... 9	1 37 0	91 57 51	91 42 53	1 39 45	2 28	
☽ Pollux	... ...	8 19 50	54 29 2	54 30 35	1 30 0		7 17
☽ Ald.	... 10	9 4 4	79 17 2	79 18 21	1 41 45	4 28	
... ...	... ...	9 27 49	79 56 19	79 30 6	1 39 0	1 43	
☽ Pollux	... 18	9 9 59	60 44 3	60 0 28	1 31 45		5 32
... ...	... ...	9 14 58	60 46 41	60 3 20	1 43 45	6 28	
☉ ... ☽	Mar. 6	23 44 44	45 53 41	45 48 11	1 34 45		2 32
... ...	... ...	23 47 55	45 55 14	45 50 30	1 39 15	1 58	
... ...	... ...	23 51 45	45 58 20	45 51 6	1 26 45		10 32
... ...	... ...	23 58 50	46 0 21	45 56 36	1 31 0		6 17
... ...	... 7	21 31 30	58 23 52	58 8 20	1 43 15	5 58	
... ...	... ...	22 17 32	58 47 57	58 32 17	1 45 45	8 28	
... ...	... ...	22 49 1	59 4 42	58 49 9	1 28 15		9 2
... ...	... ...	23 19 11	59 20 57	59 6 26	1 42 0	4 43	
... ...	... ...	23 46 20	59 35 1	59 21 13	1 45 45	8 28	
... ...	... 8	1 46 2	60 31 1	60 27 59	1 39 15	1 58	
... ...	... ...	1 53 12	60 31 18	60 24 17	1 43 45	6 28	
... ...	... 9	23 21 28	85 25 30	84 59 10	1 44 15	6 58	
... ...	... 10	1 52 8	86 39 45	86 18 8	1 44 0	6 43	
☽ Pollux	... ...	8 9 11	31 27 10	31 4 39	1 35 30		1 47
... ...	... ...	8 17 20	31 23 45	30 59 45	1 37 30	0 13	
☉ ... ☽	... 12	1 49 55	111 5 56	110 31 49	1 39 30	2 13	
... ...	... ...	2 45 50	111 31 40	110 59 13	1 39 45	2 28	
☽ Regulus	... ..	8 24 46	41 10 59	41 40 41	1 40 45	3 28	
... ...	.. 13	8 15 10	29 0 18	29 11 29	1 26 45		10 32
... ...	... ...	9 3 7	28 41 7	28 46 11	1 38 30	1 13	
... ...	.. ...	9 9 59	28 38 29	28 42 45	1 34 45		2 32
☽ Ald.	... 14	7 8 15	63 38 27	63 18 7	1 25 45		11 32
... ...	... ...	7 38 15	63 50 12	63 33 31	1 36 30		0 47
... ...	... ..	7 51 36	63 51 57	63 39 53	1 28 0		9 17
... ...	... ...	9 45 47	61 37 12	61 37 31	1 41 15	6 58	
☽ Spica	... ..	9 50 31	69 49 5	70 0 8	1 44 15	6 58	
☽ Ald.	... ...	9 55 8	61 40 31	61 41 55	1 36 30		0 47
☽ Spica	... ...	9 59 17	69 46 15	69 57 47	1 40 45	3 28	
☽ Pollux	... 16	7 10 37	44 30 31	43 49 57	1 32 15		5 2
... ...	.. ...	7 19 5	44 34 25	43 54 33	1 43 30	6 13	
... ...	... ...	8 16 38	44 58 0	44 22 55	1 34 15		3 2
... ...	... ...	8 31 10	45 3 55	44 30 12	1 38 0	0 43	
... ...	... ...	10 2 10	45 39 54	45 15 54	1 40 15		2 58
				Mean long.	1 37 17		

It will be observed, that out of the above fifty independent sets of observations, taken at different times during nearly six weeks, the deviation of any one from the mean of the whole, *in one instance only*, amounts to twelve miles; and that the average deviation, either in excess or defect, is less than five miles. It will be observed also, that the longitudes obtained by distances from stars are quite as uniform as those deduced from distances of the sun. The observations comprise the whole that were taken in the interval between the first and the last of the days of observation; not one has been omitted. Their results may therefore be considered as a fair specimen of what, under ordinary circumstances, the method is capable of effecting; and making every reasonable allowance, we may infer from them with considerable confidence, that a practised observer will generally determine his longitude by one set of lunar distances to within less than ten miles; and that it is quite improbable that the error of any one set will ever considerably exceed a quarter of a degree. The mean result of a number of independent sets, will of course reduce the probable error within still narrower limits.

The following distances, measured on the 8th and 9th of May, were taken under extremely favourable circumstances; and the two observations made on June 26th were made, when, from the situation of the School with respect to the smoke of the town, and the smallness of the moon's illumined disk, she could not be seen with the naked eye. It will be observed that the deviations from the mean are here very considerably smaller than those in the former series.

Objects observed.		Day.		App. Time at Newcastle.	App. Dist.	True Dist.	Long. W.
				H. M. S.	° ′ ″	° ′ ″	° ′ ″
☉ and	☽	May	8	2 6 21	86 4 13	85 28 37	1 14 0
...	...	...	...	2 10 0	86 5 42	85 30 22	1 36 45
...	...	...	...	2 45 21	86 18 46	85 16 6	1 35 15
...	...	...	...	4 19 5	86 54 43	86 31 56	1 38 45
...	...	...	...	4 26 34	86 57 2	86 35 40	1 43 0
...	...	...	...	4 53 44	87 6 17	86 18 27	1 55 45
...	...	...	...	4 55 56	87 7 2	86 49 30	1 36 0
☽	Spica	...	...	8 41 8	65 1 28	64 57 42	1 37 0
...	...	...	...	8 59 17	64 55 0	64 49 49	1 38 30
...	...	...	...	9 41 16	64 36 59	64 28 9	1 38 0
...	...	...	...	9 47 56	64 33 51	64 24 48	1 35 30
...	...	...	...	9 51 2	64 31 0	64 21 40	1 35 0
...	...	...	...	9 59 39	64 28 20	64 18 34	1 40 45
☉ ...	☽	...	9	2 27 47	97 37 49	96 59 10	1 31 15
...	...	...	...	2 37 4	97 41 36	97 3 32	1 33 15
...	...	...	...	2 49 34	97 46 58	97 9 20	1 34 0
...	...	...	...	3 0 37	97 51 5	97 14 31	1 34 15
...	...	...	...	3 14 39	97 56 38	97 21 9	1 37 45
...	...	June	26	2 11 25	41 29 20	40 50 42	1 39 45
...	...	...	...	2 34 28	41 17 18	40 38 3	1 37 0

If we suppose that only ten such observations could be obtained on a voyage from America to Europe, the distance would then be divided into parts of two or three hundred miles in length, at the end of each of which the place of the ship would be known with probably greater exactness than the geographical situation of Tinmouth was till towards the close of the last century. And if the mariner were provided with a chronometer to connect his lunar observations with each other, he would at all times have such a knowledge of his situation, as would enable him to take the best measures for bringing his voyage to a successful issue.

Enough has, perhaps, been said in reply to those who surmise that the lunar method of finding the longitude is uncertain in its results. It is hoped also, that enough has been said to convince those connected with the shipping interests engaged in the general foreign trade of the country, of the great importance of the modern improvements in nautical science; and of the expediency of requiring in those intrusted with the charge of their property at sea, a more intimate acquaintance with those improvements than at present they generally possess.

That a ship on being sent to sea is furnished with a sufficient stock of unexceptionable materials, the merchant and the insurer are generally very careful to ascertain; but it is in comparatively few cases indeed, that any inquiry is made respecting the qualifications of the person who is to command her, though, in estimating the risk of the voyage, those qualifications are of paramount importance.

That the master of a ship should be sober in his habits, collected and ready in circumstances of difficulty, perfectly acquainted with the management of a ship, and with the local dangers of the coasts which he may have occasion to visit, is too obvious to require remark. The importance of these primary qualifications for the office of a master mariner is universally admitted. Besides these qualifications, however, a master employed in the foreign service should be acquainted with every branch, either of art or of science, that may either enable to determine his situation *at sea*; or contribute in any way to the speedy and successful termination of his voyage.

But many, *very many* masters of merchantmen are perfectly unacquainted with many important branches of the art which it is their business to practise. Of those among them who have so far acquainted themselves with the great modern improvements in navigation as to be able to reduce them to practice, the number is surprisingly limited; and a great majority of them follow, without improvement or alteration, the practice of their predecessors who a hundred years ago were engaged in the same employment. Nautical Tables, Charts, and Instruments for observation have indeed been improved; but the general system of nautical practice has continued the same; the instruments have been applied to no new purpose; not one common merchantman in twenty is furnished even with a Nautical Almanac.

Amidst all this inattention, those ship-owners who have the discernment to employ such men only in navigating their vessels as are perfectly masters of their profession, would do well to reflect, that in connecting themselves with an ordinary ensurance club, they are not ensured upon equitable principles. If their ships are better navigated than those of other persons ensured in the same club, they contribute to ensure those persons against a greater risk than they require to be ensured against themselves Such gentlemen can surely require no argument to convince them, that any measure tending to elevate the general average of nautical information is worthy of attentive consideration. And one would imagine that the immense sums which are annually paid as ensurance for losses of which the causes cannot be satisfactorily ascertained, would excite even less intelligent persons to inquire whether so serious an evil might not in any way be remedied.

No expedient that can be devised, will impart entire security to property exposed to the dangers of the ocean; but the risks of loss would undoubtedly be diminished, if greater attention were paid to the qualifications of those to whose charge the business of navigation is committed.

To effect this desirable object, nothing more seems necessary, than that, on the return of any ship from a foreign voyage, a complete copy of such parts of the log as relate to the navigation, should be delivered to the club or society in which she may have been ensured. Masters would then be stimulated to exert themselves, so that the exhibition of their skill on such occasions might do them no discredit; and even those in whom the habit of blundering and confusion had become most rooted, might be expected to make some efforts to preserve their professional credit with the gentlemen who would thus have the means of estimating it correctly.

But to render this measure completely effectual, persons competent to the task should be deputed to examine the log of every ship; and to report on the manner in which the navigation had been conducted. It ought to be the object of particular inquiry, whether *from want of skill in nautical science* the mariner had ever been uncertain respecting the situation of the ship, to an extent which materially affected the risk of the voyage; and whether, at any rate, higher professional attainments than he appeared to have made, would not have enabled him to discharge his duty in a manner still more satisfactory.

Besides observing whether the common reckoning from the log, and the compass, had been properly attended to; it ought to be examined whether the ship's place had been determined by appropriate observations on every suitable occasion; whether, when observations of the sun for the latitude could not be obtained, their place had been supplied by observations of the moon or some other celestial object; whether the variation of the ship's compass had been determined by actual observation; and whether such observations as had been made for the longitude, appeared to have been made in a satisfactory manner.

If the log of every ship were thus subjected to review, we should soon see the masters of British merchantmen as much distinguished for their science as they are now for their seamanship.

A few years will introduce into efficient situations in the merchant service, many individuals whose opportunities of acquiring professional information, while they have been greater than many of their predecessors enjoyed, have also been assiduously improved. It is to be hoped that, while by pursuing an upright and manly course of conduct they will secure the esteem of all with whom they may be connected, the effect of their example in exciting a better spirit among their brethren will not be inconsiderable.

But unless some decisive general measure be adopted by those whose interests are so essentially connected with the state of nautical science, it will be a considerable time before the effect of example, or the efforts of isolated individuals, however intelligent or influential, will produce a material change in the habits of so large a body of men.

It cannot, however, be too frequently repeated, that to acquire a thorough practical knowledge of every useful branch of nautical science, neither much time nor great talent is requisite. Those seamen, therefore, who may continue to content themselves with the clumsy, unscientific practice of former ages, can have no rational apology to offer to the public. In what way they may excuse their conduct to themselves, it is of little importance to conjecture.

LXXII. *An Account of the Comparison of various British Standards of linear Measure. By Capt.* Henry Kater, *F.R.S. &c.*

[Concluded from p. 298.]

It may be seen in the former part of this paper, that the temperature at which the points were laid off on Mr. Ramsden's bar from the brass scale, was 54°; consequently, the observed lengths of the brass scales and 40-inch bar, must be reduced to this temperature. The expansion of one foot of General Roy's scale for one degree of the thermometer was found by him to be ·0001237, and of one foot of Ramsden's bar ·0000740 of an inch; consequently, the excess of the expansion of 40 inches of the scale, above 40 inches of the bar, for each degree above 54° will be ·0001657 of an inch; and this quantity has been used in computing the corrections for temperature.

The comparison of both scales with the bar was made at the same time; but to avoid confusion, I have given the results in separate tables. The scales and the 40-inch bar were laid together two days previously to commencing the examination.

TABLE I.

Comparisons of the Distance from Zero to 40 Inches of General ROY'S *Scale with the 40-inch Bar.*

Date.	Temp.	Reading.		Difference between the scale and the bar in inches.	Correction for Temperature.	Roy's scale *shorter* than the 40-inch bar.
		Bar.	Roy.			
May 7	59·0	23	32·5	—·000407	—·000829	·001236
8	62·0	11	23	—·000385	—·001326	·001711
9	65·0	11·5	43·5	—·000086	—·001823	·001909
	65·2	41	40	+·000043	—·001856	·001813
	65·3	30	26	+·000171	—·001873	·001701
	65·3	30	29	+·000043	—·001873	·001829
	64·7	27	25	+·000086	—·001773	·001687
10	67·1	36·2	24	+·000522	—·002220	·001698
					Mean	00·1698

By the above comparisons, General Roy's slale appears to be shorter than the 40-inch bar ·001698; to which adding ·000034, the quantity by which Ramsden's bar exceeds the 40-inch bar, we have ·001732 of an inch for the difference *in defect* between General Roy's scale and the standard used in the Trigonometrical Survey, with which it was supposed to be identical.

TABLE II.

Comparisons of the Distance from Zero to 40 Inches of Sir GEORGE SHUCKBURGH'S *Scale with the 40-inch Bar.*

Date	Temp.	Readings.		Difference between the scale and the bar in inches.	Correction for Temperature	Shuckburgh's scale *shorter* than the 40-bar.
		Bar.	Shuck.			
May 7	59·0	23	75·2	—·002234	—·000829	·003063
9	65·0	41·5	62	—·000877	—·001823	·002700
	65·2	41	58	—·000728	—·001856	·002584
	65·3	30	49	—·000813	—·001873	·002686
	65·3	30	51	—·000899	—·001873	·002772
	64·7	27	50	—·000984	—·001773	·002757
10	67·4	36·2	11·5	—·000227	—·002220	·002447
	67·8	33	41	—·000471	—·002267	·002738
12	67·5	42	48	—·000257	—·002237	·002494
					Mean	·002696

If to the above mean ·000084 be added as before, we have ·00273 of an inch, by which the distance from zero to 40 inches of Sir George Shuckburgh's scale is shorter than one sixth part of Ramsden's bar.

The very great difference between Ramsden's bar and General Roy's scale, made me desirous of comparing this last with the Royal Society's standard; and as I was aware of the existence of other standards of considerable importance, I resolved to examine them at the same time.

The Royal Society's scale has been described by Sir George Shuckburgh: it is of brass, and about the same dimensions as General Roy's scale, which is already well known. It has three parallel lines drawn upon it lengthwise. On one of the exterior lines marked E, are two dots expressing the length of the Tower yard. This is the yard which has been heretofore called, and which I shall still call, *the Royal Society's standard*. The middle line has the Exchequer yard marked upon it; and the other exterior line has dots, at precisely the same distance as those of the Royal Society's standard.

Knowing that Mr. Cary had made for Lieutenant-Colonel Lambton a standard scale, which forms the basis of the Trigonometrical Survey carried on by him in India, and aware of the importance of ascertaining the value of this in parts of other known standards, I inquired of Mr. Cary whence it was derived, and was informed that it had been copied from a scale then in the possession of Alexander Aubert, Esq., and which, after his death, was purchased by Mr. Jones, of Holborn. On application being made by the Commissioners of Weights and Measures to Mr. Jones for the loan of it, their request was readily and obligingly complied with.

This scale is of plate brass, strengthened by an edge bar: it contains 61 inches, and has the name of Bird upon it. Two dots upon two gold pins designate the yard, from which the divisions of the scale have evidently been derived. There is also a third dot, marking, I believe, the length of the French half toise. The dots indicating the yard are those I employed. I shall call this scale *Colonel Lambton's standard.*

Bird's Parliamentary standard yard of 1758 had already been compared with Sir Geo. Shuckburgh's scale by him, and recently by myself, and found to exceed it about two ten-thousandths of an inch. In Sir George Shuckburgh's "Account of experiments for determining a standard of weights and measures," he remarks, that there existed another standard yard made by Bird, in the year 1760, which did not differ more than two ten-thousandths of an inch from the standard of 1758; but he does not say

whether

whether this difference was observed to be in excess or in defect.

As it was possible that this standard yard of 1760 might coincide with 36 inches of Sir George Shuckburgh's scale, I was anxious to compare them together, and by the exertions of Davies Gilbert, Esq., M.P., the standard of 1760 was found in the custody of the House of Commons, and confided to my care. It is (as Sir George Shuckburgh observed) precisely similar in form to the standard of 1758, the yard being marked by two dots upon gold pins, which, though very large, are in tolerable preservation.

The five standard scales which I have just described, were placed together on the 15th of June 1820, and arranged so that those of least bulk should be furthest from me during the observations, as they would be more readily affected by the proximity of the person of the observer.

As I was desirous that comparisons of such importance should not rest wholly on my own authority, I requested Dr. Wollaston to take two series of measurements, which, together with my own, are contained in the following table.

Table III.

Comparisons of various Standards.

Date. 1820.	Temperature.	Readings of the micromoter at				
		The Royal Society's standard.	General Roy's scale.	Sir Geo. Shuckburgh's scale.	Bird's standard of 1760.	Colonel Lambton's standard.
June 16	61·5	19	23	43	43	66
	—	6	17	41·5	41·5	58
	—	8	14	36	36	50
	—	86	92	118	118	133
	64·0	75	93	114	114	121
	—	82	91	111	106	126
	—	78	93	112	108	127
	63·5	75	93	113	110	124
17	61·5	15	24	44	40	59
	—	7	17	39	36	53
	—	1	14	35	36	45
	64·5	59	70	94	94	115
(By Dr. Wollaston) 18	64 5	30	39	64	64	76
(By ditto	—	21	36	56	56	66
Mean of the whole...............		40·1	51·1	72·9	71·6	87

I now returned to the forty-inch iron bar and General Roy's scale, anxious to verify my former conclusion by a fresh examination. The microscopes being fixed at the proper distance, comparisons were made, which I shall detail before I state the results afforded by the preceding table.

TABLE IV.

Further Comparisons of the Distance from Zero to Forty Inches of General ROY'S *Scale, with the Forty-inch Bar.*

Date.	Temp.	Readings.		Difference between the scale and the bar in inches.	Correction for Temperature.	Roy's scale *shorter* than the 40-inch bar.
		Bar.	Roy.			
June 19	64·5	29	30	— ·000043	—·001740	·001783
	65·0	28	26	+ ·000086	—·001823	·001737
	65·5	30	30	—·000000	—·001906	·001906
	66·7	12	5	+ ·000300	—·002104	·001804
	65·2	93	89	+ ·000171	—·001856	·001685
20	61·7	0	13	— ·000556	—·001276	·001832
	—	2	11	—·000514	—·001276	·001790
	—	0	11	— ·000599	—·001276	·001875
	—	1·5	15	— ·000577	—·001276	·001853
	—	3	16	—·000556	—·001326	·001882
					Mean	·001817

Adding to the mean thus obtained 000034, the excess of one sixth of Ramsden's bar above the forty-inch bar, we have ·001849 of an inch for the excess of 40 inches of the standard used in the Trigonometrical Survey, above General Roy's scale, differing from the result given by the former comparisons contained in Table I. only ·000117 of an inch, a difference which may be attributed to uncertainty of temperature. The mean of both ·00179, is probably very near the truth.

I shall now proceed to give in one view, the results deduced from Table III. by comparing each standard in succession, with that used by Colonel Lambton in the survey of India.

Excess of the following standards above Colonel Lambton's standard.	On 36 inches.
Sir G. Shuckburgh's standard	+·000642
Bird's standard of 1760	+·000659
General Roy's scale	+·001537
Royal Society's standard	+·002007
Ramsden's bar (used in the Trigonometrical Survey of Great Britain)	+·003147

If the results of the two series of comparisons made by Dr. Wollaston be examined, it will be seen that the greatest difference from those above given, is not two ten-thousandths of an inch, and this difference appears to have arisen almost wholly from the ill defined dots of the Royal Society's standard.

The standard used in the Trigonometrical Survey, being thus unexpectedly found to differ so considerably from every other standard of authority, the Commissioners of Weights and Measures

sures proposed in their Second Report, that Bird's Parliamentary standard of 1760 should be considered as the foundation of all legal weights and measures.

It may be seen, that the standard thus selected, differs so little, if at all, from that of Sir George Shuckburgh, that they may, for every purpose, be considered as perfectly identical; and this agreement is particularly convenient, because the length of the mêtre having been determined by comparisons with Sir Geo. Shuckburgh's scale, and a fac simile of it made by Mr. Troughton, for Professor Pictet, all measures on the Continent are converted into English measure, by a reference to Sir George Shuckburgh's standard.

In determining the figure of the earth, by means of the measurement of distant portions of the same meridian, many anomalies have been remarked, which may, in some instances, be attributed to the difference of the standards employed in such measurements. As an example of the importance of this consideration, I shall examine the results deduced by Lieutenant-Colonel Lambton, from a comparison of three sections of the great arc measured by him in India, with the lengths of the French, the English, and the Swedish degrees. The abridgement of Lieutenant-Colonel Lambton's very important operations may be found in the Philosophical Transactions for 1818.

The following are the data given by Colonel Lambton.

The length of the degree due to

Lat. $9^{\circ}\ 34'\ 44''$ is 60172·83 fathoms.
13 2 55 60187·56
16 34 42 60512·78

By the French measurement, in
Lat. 47 30 46 60779 fathoms.

By the English, in
Lat. 52 2 20 60820

By the Swedish, in
Lat. 66 20 12 60955

and by successively comparing the lengths of the European degrees with the three sections of the Indian arc, Colonel Lambton obtains for the compression

By the French $\frac{1}{305{\cdot}73}$ $\frac{1}{306{\cdot}}$ $315{\cdot}03$ mean 309.15

By the English $\frac{1}{310.28}$ $\frac{1}{11{\cdot}36}$ $\frac{}{318{\cdot}97}$ mean $\frac{1}{313.54}$

By the Swedish $\frac{}{305{\cdot}14}$ $\frac{1}{305{\cdot}72}$ $\frac{1}{310{\cdot}72}$ mean $\frac{1}{307{\cdot}19}$

and the mean of the three means $= \frac{1}{309.96}$.

In order to reduce the preceding measurements to the English national standard, we have to multiply the fathoms of the Indian degree by —·000018, and of the English by +·00007, to obtain the correction to be applied, with its proper sign, to the length of the degree. The French and Swedish degrees require no correction.

We have then the following data for computation:

By sections of the Indian arc	9° 34′ 41″	60471·74 fathoms.
	12 2 55	60486·47
	16 34 42	60511·69
The French	47 30 46	60779
English	52 2 20	60824·25
Swedish	66 20 12	60955

and the resulting compression

$$\text{By the French}\quad \frac{1}{304\cdot64}\quad \frac{1}{305\cdot55}\quad \frac{1}{313\cdot77}\quad \text{mean}\ \frac{1}{307\cdot99}$$

$$\text{English}\quad \frac{1}{305\cdot57}\quad \frac{1}{306\cdot40}\quad \frac{1}{313\cdot50}\quad \text{mean}\ \frac{1}{308\cdot49}$$

$$\text{Swedish}\quad \frac{1}{301\cdot11}\quad \frac{1}{305\cdot01}\quad \frac{1}{309\cdot09}\quad \text{mean}\ \frac{1}{307\cdot55}$$

and the mean of the three means = $_{307\cdot55}$.

As it appears that the compression obtained by employing the length of the degree in lat. 16° 34′ 42″ is uniformly in defect, whilst the results deduced from the other two sections are very nearly alike, it might perhaps be allowable to consider $\frac{1}{305\cdot32}$, the mean of these last results, as the true compression; and this would agree very nearly with the deduction of M. Laplace, from the lunar irregularities; with the result of Dr. Young's interesting and novel investigation, by a comparison of the mean, with the superficial density of the earth; and with the conjecture I have hazarded from the compression given by the experiments on the length of the pendulum at Unst and Portsoy.

August 3, 1820.

LXXIII. *On* MAYER'S *Formula for the astronomical Refraction.* *By* JAMES IVORY, *M.A. F.R.S.*

THE rule of Mayer for calculating the refraction of the stars was published with his Lunar Tables; but without demonstration, or any hint of the manner in which it was investigated. Since that time the formula has not been explained in a very satisfactory manner, and must still be considered as a problem to be solved*.

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* "La règle de Mayer, proposée sans démonstration dans ses Tables de la Lune, est encore aujourd'hui un probleme à résoudre, parce qu'il a laissé ignorer le chemin qui l'y a conduit."—KRAMP. *Refractions Astron.* p. 169.

It is not essentially different from the other rules which astronomers have deduced from the hypothesis which, next to the supposition of a constant density, suggests itself as the most simple; namely, that of a density decreasing uniformly as the height above the earth's surface increases: but it is distinguished from all the other rules by the manner in which allowance is made for the variations of the thermometer. In this respect chiefly Mayer's formula seems deserving of discussion: because, when we depart from the usual procedure of astronomers, of correcting the mean refractions in proportion to the density of the air, all the other methods that have been proposed are arbitrary, depending upon particular suppositions, or empirical, deriving authority from observation alone.

The rule of Mayer is contained in these two formulæ, viz.

$$\text{Tan } y = \frac{(1+mt)^{\frac{1}{2}}}{16\cdot 5\sqrt{\cos A}},$$

$$r = \frac{1982''\cdot 2 \times \tan \frac{1}{2} y}{(1+mt)^{\frac{3}{2}}} \times \frac{b}{B}:$$

in which A is the zenith distance; the factor $1+mt$, the correction for the variation of temperature; t denoting the degrees of the centigrade thermometer; b the height of the barometer actually observed; and B the mean height of 0·76 metres, or 29·92 English inches.

Of this formula, which is taken from the introduction to the Astronomical Tables published by the French Board of Longitude, Delambre observes:

"Ce qui distingue cette formule de toutes les autres, c'est l'exposant $\frac{1}{2}$ au lieu de 0, dans l'expression de tan y, et l'exposant $\frac{3}{2}$ au lieu de 1, dans celle de r. Si cette difference semblait avantageuse, on pourrait l'introduire également dans les formules précédentes. En la supprimant dans la formule de Mayer, on la réduirait à l'expression

$$r = 59''\cdot 98 \times \frac{\tan (A - 3\cdot 15r)}{1+mt} \times \frac{b}{B}.$$

Jusqu'à 70°, cette formule ne differe de la précédente que de 0″·1, ou 0″·2 tout au plus. Vers 80°, les differences vont jusqu' à 1″·8 à 30° du thermometre; à 85°, elles montent à 5″·9; à l'horison la difference va jusqu'à 92″. L'effet des exposants $\frac{1}{2}$ et $\frac{3}{2}$ de Mayer est donc nul presque toujours, et quand il devient sensible, il m'a paru peu conforme aux observations."

The expression of the horizontal refraction, according to Mayer, is $\frac{1985''\cdot 2}{(1+mt)^{\frac{3}{2}}} \times \frac{b}{B}$.

There is no difficulty with regard to the barometer; but the correction for heat is different from that usually employed; and, we see that in this respect the rule is not approved of by the illustrious astronomer whose words have just been quoted. The want of a sufficient number of accurate determinations of the horizontal refraction at different temperatures, makes it impossible to decide this point by the test of observation: but we may examine it by the light of theory; and, in this manner, I shall now prove that, in every possible hypothesis relating to the constitution of the atmosphere, the factor of Mayer depending on heat, viz.

$$\frac{1}{(1+mt)^{\frac{3}{2}}},$$

ought to enter into the expression of the horizontal refraction.

The most simple hypothesis is that of Cassini, who supposed an atmosphere of the same uniform density throughout. Let a denote the radius of the earth; and l, the height of the homogeneous atmosphere. It is to be observed, that l will be of the same length so long as the temperature at the earth's surface remains constant, although the pressure vary. This will be obvious, if it be considered that the weight of a column of air of the length l will change in the same proportion as its density; that is, as the pressure at the earth's surface. But, the pressure remaining the same, if the temperature vary, the length of l will increase or decrease inversely as the changes of density. According to Laplace, l is equal to 7974 metres, or 4360 fathoms, at the temperature of melting ice; and, for a variation of temperature of t degrees from that fixed point, its length will be $4360 \times (1+mt)$.

Suppose that a ray of light from a star falls upon the surface of the sphere bounding the homogeneous atmosphere, and is there refracted in a straight line to the eye of a spectator on the earth: put y for the perpendicular falling upon the refracted ray from the earth's centre; and ϕ for the angle of refraction, that is, the angle between the refracted ray and the radius of the sphere drawn to the point of incidence: then

$$(a+l)\sin\phi = y.$$

But, if A denote the apparent zenith distance of the star, we shall likewise have

$$a\sin A = y. \quad \text{Wherefore,}$$

$$(a+l)\sin\phi = a\sin A,$$

$$i = \frac{l}{a},$$

$$\operatorname{Sin}\phi = \frac{\sin A}{1+i},$$

$$\operatorname{Cos}\phi = \frac{\sqrt{\cos^2 A + 2i + i^2}}{1+i},$$

$$\operatorname{Tan}\phi = \frac{\sin A}{\sqrt{\cos^2 A + 2i + i^2}}.$$

It

If r denote the refraction of the star, the angle of incidence of the ray of light will be $\phi + r$; wherefore, supposing that $1 + \beta$ to 1 is the constant ratio of the sine of incidence to the sine of refraction, we shall have

$$\text{Sin}\,(\phi + r) = (1 + \beta)\sin\phi,$$

$$\text{Sin}\,\phi\cos r + \sin r\cos\phi = (1 + \beta)\sin\phi,$$

$$\text{Sin}\,r = \beta\tan\phi + (1 - \cos r).\tan\phi.$$

It will be abundantly accurate, even at the horizon, to put $r - \frac{r^3}{6}$ for $\sin r$, and $\frac{1}{2}r^2$ for $1 - \cos r$, and then by seeking the value of r from the resulting expression we shall get

$$r = \beta\tan\phi + \tfrac{1}{2}\beta^2\tan^3\phi + \beta^3 \times \left\{\frac{\tan^5\phi}{2} + \frac{\tan^3\phi}{6}\right\}.$$

In this expression $\frac{\beta^3\tan^5\phi}{2}$ becomes sensible at the horizon; but $\frac{\beta^3\tan^3\phi}{6}$ may always be neglected: wherefore, by substituting the value of $\tan\phi$, we shall finally get

$$r = \frac{\beta\sin A}{\left\{\cos^2 A + 2i + i^2\right\}^{\frac{1}{2}}}$$

$$+ \frac{1}{2}\cdot\frac{\beta^2\sin^3 A}{\left\{\cos^2 A + 2i + i^2\right\}^{\frac{3}{2}}},$$

$$+ \frac{1}{2}\cdot\frac{\beta^3\sin^5 A}{\left\{\cos^2 A + 2i + i^2\right\}^{\frac{5}{2}}}.$$

At the horizon, $\sin A = 1$, and $\cos A = 0$; and, omitting insensible quantities, we obtain

$$r = \frac{\beta}{\sqrt{2i}} \times \left\{1 + \frac{1}{4}\cdot\frac{\beta}{i} + \frac{1}{8}\cdot\frac{\beta^2}{i^2}\right\}.$$

If the values of β and i given by Laplace* be substituted in this expression, the horizontal refraction will come out 1290″, which agrees with the determination in the *Mécanique Céleste*, vol. iv. p. 246.

The quantity of r will vary as β and i change with the state of the atmosphere. Now, β is always proportional to the density of the air: it varies, therefore, directly with the height of the mercury in the barometer, and inversely with the thermometrical changes. The standard height of the barometer being B, when the actual height is b, and the actual temperature t, the value of β will become

$$\frac{\beta}{1 + mt} \times \frac{b}{B}.$$

The other quantity $i = \frac{l}{a}$, varies with the thermometer only, as has already been shown: at the temperature t, it becomes

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* See Phil. Magazine for last September, p 167

$$i \times (1+mt).$$

On the whole, therefore, by the changes of the barometer and thermometer, the factor $\frac{\beta}{\sqrt{2i}}$, which enters into the expression of the horizontal refraction, will become

[illegible]

and this proves incontestably the exactness of Mayer's rule in the hypothesis of Cassini. The other factor into which $\frac{\beta}{\sqrt{2i}}$ is multiplied, will likewise produce some changes in the horizontal refraction; but the variation arising from this cause is not very considerable; it is peculiar to the particular hypothesis; and it is not now the subject of consideration.

The hypothesis of Cassini, on account of its simplicity, is well calculated to elucidate the variations in refraction produced by barometrical and thermometrical changes. As is apt to be the case in some more complicated suppositions, the attention is not here so absorbed by intricate combinations of the quantities concerned, as to overlook the manner in which the phænomenon is produced by its real causes, which are the alterations in the refracting power of the medium, and in the extent of the homogeneous atmosphere. But, if we attend to the analysis of this problem, given in this Magazine for September last, it will appear that the same factor $\frac{\beta}{\sqrt{2i}}$, which enters into the expression of the horizontal refraction in the hypothesis of Cassini, is equally a part of the same quantity in every hypothesis. We must, therefore, conclude that the rule of Mayer is true, not only in the hypothesis of Cassini, or in that of Mayer whatever it was, but absolutely in every supposition that can possibly be formed with regard to the constitution of the atmosphere.

It follows from the nature of the differential expression of the refraction that, so long as the zenith distance is not too great; or so long as $\cos^2 A$ is considerably greater than i; we may develop the refraction in a series of terms multiplied by the small coefficients, β; β^2, $i\beta$, &c., the quantity i never entering into a denominator. Now the term multiplied by $i\beta$ varies only with the barometer; for heat produces equal and opposite changes in i and β, which compensate another: and, on the whole, so far as this mode of computation can be followed, the variation in refraction is very nearly proportional to β, that is to the density of the air, according to the usual practice of astronomers. But when the zenith distance is great; or when $\cos^2 A$ is less than

i; another mode of calculation must be pursued; the quantity i necessarily becomes a divisor; and the whole expression acquires the factor $\frac{\beta}{\sqrt{i}}$, which necessarily leads to the rule of Mayer.

Mayer's formula was, no doubt, investigated by means of the hypothesis of a uniform decrease of density in ascending from the earth's surface; and, for the sake of further illustration, we may deduce it from the analysis in the Magazine for September. The hypothesis mentioned is contained in the equation $fw = s$, f being a constant quantity; for w is the decrease of density, and s, the height ascended divided by l. If fw be substituted for s in the equation of the pressure, we shall obtain,

$$\frac{y}{y} = 1 - fw + \frac{fw^2}{2};$$

and, as this expression must vanish when $w = 1$, we get $f = 2$. At the boundary of the atmosphere, the equation $s = 2$ takes place, so that the total height is equal to $2l$.*

It must be observed, however, that in a limited atmosphere, the ultimate density, $1 - w$, is not, strictly speaking, evanescent; but equal to some small quantity, less than what would obtain at an equal height in an atmosphere of uniform temperature.

Now, if $2w$ be substituted for s in the formula for the refraction, we get

$$dr = \frac{\beta \sin A \, dw}{\sqrt{\cos^2 A + (4i - 2\beta) w}}.$$

To integrate this expression, assume

$$w = (1 - e^2) u + e^2 u;$$

then, Δ being the radical quantity in the denominator, we have

$$\Delta = \sqrt{\{\cos^2 A + (4i - 2\beta)(1 - e^2) u + (4i - 2\beta) e^2 u^2\}};$$

and, by determining e^2 so as to make the expression on the right-hand side a square,

$$\frac{\sqrt{4i - 2\beta}}{\cos A} = \frac{2e}{1 - e^2};$$

$$\Delta = \cos A + e u \sqrt{4i - 2\beta},$$

$$\frac{dw}{\Delta} = \frac{\text{[illegible]} u}{\sqrt{4i - 2\beta}};$$

$$dr = \sin A \times \frac{2\beta}{\sqrt{4i - 2\beta}} \times e du.$$

Now, w and u increase together from zero to 1; wherefore

$$r = \frac{2\beta}{\sqrt{4i - 2\beta}} \times \sin A \times e:$$

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* Mecanique Céleste, vol. iv. p. 260.

or, if we make

$$\text{Tan}\, y = \frac{\sqrt{4i - 2\beta}}{\cos A};$$

$$r = \frac{2\beta}{\sqrt{4i - 2\beta}} \times \sin A \tan \tfrac{1}{2} y.$$

These expressions take place at the standard temperature and mean pressure; but, according to what has been shown, and neglecting in the radical quantity the variation of 2β which is inconsiderable when compared with the variation of $4i$, they will become, at the temperature t, and the pressure b,

$$\text{Tan}\, y = (1 + mt)^{\frac{1}{2}} \times \frac{\sqrt{4i - 2\beta}}{\cos A},$$

$$r = \frac{1}{(1 + mt)^{\frac{3}{2}}} \times \frac{b}{B} \times \frac{2\beta}{\sqrt{4i - 2\beta}} \times \sin A \tan \tfrac{1}{2} y.$$

The horizontal refraction, when $t = o$, and $b = B$, is

$$r_{,} = \frac{2\beta}{\sqrt{4i - 2\beta}};$$

which agrees with the determination in the *Mécanique Céleste*, vol. iv. p. 260, the symbols only being different. Using the values of i and β given by Laplace *, this quantity falls greatly short of observation. But the expressions that have now been investigated are perfectly similar to the formulæ of Mayer; and it is reasonable to suppose that that astronomer followed the usual practice, and determined the two coefficients so as to represent the refractions observed at the horizon, and at the altitude of 45°.

When the zenith distance is not too great, the factor depending on heat in the value of tan y, very nearly compensates the equal divisor in the expression of the refraction; and in such cases Mayer's formula coincides with the usual practice of astronomers in making the refraction vary in proportion to the density of the air. At the horizon the compensation ceases to take place, and the correction for heat, according to Mayer, is quite different from the common rule. But the formula of the celebrated astronomer of Gottingen has the merit of being, in both circumstances, consonant to general principles, and independent of arbitrary assumptions. Its imperfection arises from the physical hypothesis employed: for the law of a uniform decrease of density cannot be that of nature; because it leads to a determination of the horizontal refraction much too small; and because it limits the atmosphere to the third or fourth part of its real extent.

Sept. 5, 1821. J. IVORY.

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* Magazine for September, p. 167.

LXXIV. *Account of the Native Copper on the Southern Shore of Lake Superior, with historical Citations and miscellaneous Remarks, in a Report to the Department of War. By* HENRY R. SCHOOLCRAFT*.

(The following Letter accompanied Mr. Schoolcraft's Report.)

Albany, Feb. 16, 1821.

To Professor Silliman.

SIR,—AGREEABLY to your request, and the permission of the Secretary at War, I inclose you a copy of my Report on the Copper Mines of Lake Superior. In preparing it, I have consulted the former travellers of the region, and, by combining their remarks with my own, endeavoured to present, in an embodied form, all the information extant upon the subject. It has been a cause of regret to me, however, that more time was not devoted to the mineralogy and geology of that section of country: but it appeared incompatible with the more important objects of the expedition, and I could only make use of the time that was allowed to me. In presenting the subject to the Secretary at War, I thought my observations would be more acceptable in a practical and business form, than as assuming the character of a scientific memoir; and in choosing an intermediate course, I have probably said more on the geology of the country than may be thought important to the statesman, and less than will be considered satisfactory to the professed geologist and scientific amateur. A few marginal notes have therefore been added, but I have been studious not to overload the original MS. in that way. I do not send the views and geological charts accompanying the Report to Mr. Calhoun, as it would be very inconvenient at the present period to copy them, and as the subject may be sufficiently understood without these embellishments.

With respect to the deductions, so far as science is concerned, it is hoped they will be read with candour, and I therefore submit them to your judgement and to that of the scientific public.

With great respect and regard,
Your most obedient servant,
HENRY R. SCHOOLCRAFT.

Vernon, Oneida Co. (N. Y.) Nov. 6, 1820.

Hon. JOHN C. CALHOUN, *Secretary at War*,

SIR,—I HAVE now the honour to submit to you such observations as have occurred to me, during the recent expedition under Gov. Cass, in relation to the Copper Mines of Lake Superior;

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* From the American Journal of Science, &c. No. 2, vol. iii.

reserving as the subject of a future communication, the facts I have collected on the mineralogy of the country explored generally.

The first striking change in the mineral aspect of the country north of Lake Huron, is presented near the head of the island of St. Joseph in the river St. Mary, where the calcareous strata of secondary rocks are succeeded by a formation of red sand-stone, which extends northward to the head of that river at Point Iroquois, producing the falls called the *Sault de St. Marie* fifteen miles below, and thence stretching northwest along the whole southern shore of Lake Superior to the Fond du Lac, and into the regions beyond. This extensive stratum is perforated at various points by up-heaved masses of granite and hornblende, which appear in elevated banks on the margin of the lake between Dead river and Presque Isle, and from the Porcupine mountains ten leagues to the west of the Ontonagon river. It is overlayed in other parts by a stratum of grey sand-stone, resembling certain varieties of grauwacke, of uncommon thickness, which appears in various promontories along the shore, and, at the distance of ninety miles from Point Iroquois, constitutes a lofty perpendicular wall upon the water's edge called the Pictured Rocks, which is one of the most commanding objects in nature. So obvious a change in the geological character of the rock strata, in passing from Lake Huron to Lake Superior, prepares us to expect a corresponding one in the imbedded minerals and other natural associations,—an expectation which is realized during the first eighty leagues, in the discovery of red hematite, prehnite, opal, jasper, sardonyx, carnelion, agate, and zeolite.

The first appearances of copper are seen on the head of the portage across Keweena point, two hundred and seventy miles beyond the Sault de St. Marie, where the pebbles along the shore of the lake contain native copper disseminated in particles varying in size from a grain of sand to a lump of two pounds weight. Many of the detached stones at this place are also coloured green by the carbonate of copper, and the rock strata in the vicinity exhibit traces of the same ore. These indications continue to the river Ontonagon, which has long been noted for the large masses of native copper found upon its banks, and about the contiguous country. This river (called Donagon on Mellish's Map) is one of the largest of thirty tributaries which flow into the lake between Point Iroquois and the Fond du Lac. It originates in a district of mountainous country intermediate between the Mississippi river and the Lakes Huron and Superior, and, after running in a northern direction for one hundred and twenty miles, enters the latter at the distance of fifty-one miles west of Point Keweena, in north latitude 46° 52′ 2″ according to the observations of Captain Douglass. It is connected by portages with the Menomonie river of Green Bay, and with the Chippeway river of the Mississippi, routes of communication occasionally travelled by the Indians in canoes. At its mouth there is a village of Chippeway Indians of sixteen families who subsist chiefly on the fish (sturgeon) taken in the river; and whose location, independently of that circumstance, does not appear to unite the ordinary advantages of Indian villages in that region. A strip of alluvial land of a sandy character extends from the lake up the river three or four leagues, where it is succeeded by high broken hills of a sterile aspect and covered chiefly by a growth of pine, hemlock, and spruce. Among these hills, which may be considered as lateral spurs of the Porcupine mountains, the Copper Mines, so called, are situated, at the distance of thirty-two miles from the lake, and in the centre of a region characterized by its wild, rugged, and forbidding appearance. The large mass of native copper reposes on the west bank of the river, at the water's edge (see Plate V. fig. 3), and at the foot of a very elevated bank of alluvion, the face of which appears at some former period to have slipped into the river, carrying with it the mass of copper, together with detached blocks of granite, hornblende, and other bodies peculiar to the soil of that place. The copper, which is in a pure and malleable state, lies in connexion with serpentine rock, the face of which it almost completely overlays, and is also disseminated in masses and grains throughout the substance of the rock *. The surface of the

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* In preparing this Report, a more particular description of the geognostic character of this mass of copper was deemed unnecessary; but in presenting it for the perusal of the amateurs of natural science, it may be proper to add—that the serpentine rock is not *in situ*, nor is it so found in any part of the regions visited. To account for its appearance in a section of country to which it is geologically foreign, it would be necessary to enter into the inquiry "by what means have the loose masses of primitive rocks been transported into secondary countries?"—an inquiry which is incompatible with the limits of this Report, and which moreover would, in itself, furnish the subject of a very interesting memoir. I will now however suggest, what has struck me in passing through that country—that the Porcupine mountains, which are situated thirty miles west, are the seat of extinguished volcanoes that have thrown forth the masses of native copper which are found (as will be mentioned in the sequel) so abundantly throughout the region of the Ontonagon. This opinion is supported by the fact that those mountains are composed (so far as observed) of granite, which is probably associated with other primary rocks, and among them serpentine—that the red sand-stone rock at their base is highly inclined towards the mountains so as to be almost vertical, and apparently thrown into this position by the up-heaving of the granite—and also, that their elevation, which has been calculated by Capt. Douglass and myself at 1800 feet above the level of Lake Superior, their conical and rugged peaks, and other appearances, are such as frequently characterize volcanic mountains.

metal, unlike most oxidable metals which have suffered a long exposure to the atmosphere, presents a metallic brilliancy*; which is attributable either to alloy of the precious metals, or to the action of the river, which during its semi-annual floods carries down large quantities of sand and other alluvial matter that may serve to abrade its surface, and kept it bright. The shape of the rock is very irregular—its greatest length is three feet eight inches—its greatest breadth three feet four inches, and it may altogether contain eleven cubic feet. In size, it considerably exceeds the great mass of native iron found some years ago upon the banks of Red River in Louisiana, and now deposited among the collections of the New-York Historical Society†, but, on account of the admixture of rocky matter, is inferior in weight. Henry, who visited it in 1766, estimated its weight at five tons‡. But after examining it with scrupulous attention, I have computed the weight of *metallic copper* in the rock at twenty-two hundred pounds. The quantity may, however, have been much diminished since its first discovery, and the marks of chisels and axes upon it, with the broken tools lying around, prove that portions have been cut off and carried away. The author just quoted observes, "that such was its pure and malleable state, that with an axe he was able to cut off a portion weighing a hundred pounds." Notwithstanding this reduction, it may still be considered one of the largest and most remarkable bodies of native copper upon the globe, and is, so far as my reading extends, exceeded only by a specimen found in a valley in Brasil weighing 2666 Portuguese pounds§. Viewed only as a subject for scientific speculation, it presents the most interesting considerations, and must be regarded by the geologist as affording illustrative proofs of an important character. Its connexion with a rock which is foreign to the immediate section of country where it lies, indicates a removal from its original bed; while the intimate connexion of the metal and matrix, and the complete envelopment of individual masses of the copper by the rock, point to a common and contemporaneous origin, whether that be referable to the agency of caloric or water. This conclusion admits of an obvious and important application to the extensive strata of serpentine and other magnesian rocks found in various parts of the globe! The Ontonagon river at this place is broad, rapid and shallow, and filled with detached masses of rock out of place, which project above the water, and render the navigation extremely difficult during the summer season. The bed of the river is upon sand-stone similar to that which supports the Palisadoe rocks upon the Hudson. There is an island nearly in the centre of the river, which serves to throw the current against the west bank where the copper reposes, and which, as it is the only wooded island noticed in the river, may serve to indicate the locality of this mineral treasure to the future inquirer.

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* This, however, is no uncommon appearance of native copper.—Ed.

† See Bruce's Mineralogical Journal, p. 124, 218.

‡ See Henry's Travels and Adventures, p. 205.

§ Philip's Mineralogy.

Several other masses of native copper have been found on this river at various periods since it has been known to Europeans, and taken into different parts of the United States and of Europe, and a recent analysis of one of these specimens, at the University of Leyden, proves it to be native copper in a state of uncommon purity, and uncombined with any notable portion either of gold or silver.

A mass of copper discovered by the Aborigines on an island in Lake Superior at Point Chegoimegon eighty miles west of the Ontonagon, weighed twenty-eight pounds, and was taken to the island of Michilimackinac some years ago by M. Cadotte, and disposed of. It was from this mass that the War Department was formerly supplied with a specimen, and from which the analysis alluded to is also understood to have been made. About eleven years ago, a trader by the name of Campbell procured from the Indians a piece of copper weighing twelve pounds, which they found on an island in Winnebago lake, about a hundred miles in a direct line east of the copper rock on the Ontonagon. This was also taken to the island of Michilimackinac, and there disposed of. Other discoveries of this metal in masses, varying from one to ten pounds, are stated to have been made on the shores of Lake Superior—the Fox river—the Chippeway—the St. Croix, and the Mississippi about Prairie du Chien, but the statements do not rest on sufficient authority to justify any particular enumeration. The existence of copper in the region of Lake Superior appears to have been known to the earliest travellers and voyagers. As early as 1689 the Baron La Hontan, in concluding a description of that lake, adds "that upon it we also find copper mines, the metal of which is so fine and plentiful that there is not a seventh part loss from the ore*." In 1721 Charlevoix passed through the lakes on his way to the Gulf of Mexico, and did not allow the mineralogy of the country to escape his observations. "Large pieces of copper," he says in speaking of Lake Superior, "are found in some places on its banks, and around some of the islands, which are still the objects of a superstitious worship among the Indians. They look upon them with veneration, as if they were the presents of those gods who dwell under the waters; they collect their smallest frag-

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[*] La Hontan's Voyages to Canada, p. 214.

ments, which they carefully preserve without however making any use of them. They say that formerly a huge rock of this metal was to be seen elevated a considerable height above the surface of the water, and as it has now disappeared, they pretend that the gods have carried it elsewhere; but there is great reason to believe that in process of time the waves of the lake have covered it entirely with sand and slime; and it is certain that in several places pretty large quantities of this metal have been discovered without being obliged to dig very deep. During the course of my first voyage to this country, I was acquainted with one of our order (Jesuits) who had been formerly a goldsmith, and who while he was at the mission of *Sault de St. Marie* used to search for this metal, and made candlesticks, crosses, and censers of it, for this copper is often to be met with almost entirely pure *."

In 1766, Capt. Carver procured several pieces of native copper upon the shores of Lake Superior, and about the sources of the Chippeway and St Croix rivers, and published an account of these discoveries in his book of travels, which has served to give notoriety to the existence of that metal in the region alluded to, without however furnishing any very precise information as to its locality or abundance. He did not, from his own account, traverse the southern shore of the lake, but states that virgin copper is found in great plenty on the Ontonagon or Copper Mine river, and about other parts of Lake Superior, and adds—"that he observed many of the small islands, particularly those on the *eastern shores*, were covered with copper ore, which appeared like beds of copperas (sulphat of iron) of which many tons lay in a small space †."

Five years after Carver's visit (A.D. 1771) a considerable body of native copper was dug out of the alluvial earth on the banks of the Ontonagon river by two adventurers of the name of Henry and Bostwick, and, together with a lump of silver ore of eight pounds weight of a blue colour and semi-transparent, transported to Montreal, and from thence shipped to England, where the latter was deposited in the British Museum after an analysis of a portion of it, by which it was determined to contain 60 per cent. of silver ‡. These individuals were connected with a company which had been formed in England for the purpose of working the copper mines of lake Superior, among whom were the Duke of Gloucester, Sir William Johnstone, and several other gentlemen of rank. They built a small vessel at Point aux Pins, six miles above the Sault de St. Marie, to facilitate their operations upon the lake, and a considerable sum of money was expended.

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* Charlevoix's Journal of a Voyage to North America, vol. ii. p. 45.

† Carver's Travels, p. 67. ‡ Henry's Travels, p. 30.

pended, first,—in exploring the northern shore of the lake, and the island of Maripeaux, and afterwards,—in the mining operations which were authorized upon the banks of the Ontonagon. These transactions will be best illustrated by a quotation from the narrative account which Henry has himself published. After returning from the Canadian shore of the lake, and passing Point Iroquois, where the silver ore was found, he observes,—" Hence we coasted westward, but found nothing till we reached the Ontonagon, where, besides the detached masses of copper formerly mentioned, we saw much of the same metal *imbedded in stone.* Proposing to ourselves to make a trial on the hill, till we were better able to go to work upon the solid rock, we built a house and sent to the Sault de St. Marie for provisions. At the spot pitched upon for the commencement of our preparations, a green-coloured water which tinges iron of a copper colour, issued from the hill, and this the miners called a *leader*. In digging they found frequent masses of copper, some of which were of three pounds weight. Having arranged every thing for the accommodation of the miners, during the winter, we returned to the Sault.

" Early in the spring of 1772 we sent a boat load of provisions, but it came back on the 20th day of June, bringing with it, to our surprise, the whole establishment of miners. They reported that in the course of the winter they had penetrated *forty* feet into the face of the hill, but on the arrival of the thaw, the clay, on which on account of its stiffness they had relied, and neglected to secure it by supporters, had fallen in;—that from the detached masses of metal, which to the last had daily presented themselves, they supposed there might be ultimately reached a body of the same, but could form no conjecture of its distance, except that it was probably so far off as not to be pursued without sinking an air shaft; and lastly,—that the work would require the hands of more men than could be fed in the actual situation of the country. Here our operations in this quarter ended. The metal was probably within our reach; but if we had found it, the expense of carrying it to Montreal must have exceeded its marketable value. It was never for the exportation of copper that our company was formed, but always with a view to the silver, which it was hoped the ores, whether of copper or lead, might in sufficient quantity contain."

Eighteen years after the failure of this attempt (1789) Mackenzie passed through Lake Superior on his first voyage of discovery into the northwest, and in his description of Lake Superior says,—" On the same side, (the south) at the river Tennagon, is found a quantity of virgin copper. The Americans, soon after they got possession of that country, sent an agent thither; and I should not be surprised to hear of their employing people to work the mine. Indeed, it might be well worthy the attention of the British subjects to work the mines on the north coast, though they are not supposed to be so rich as those on the south *."

The attention of the United States government appears first to have been turned toward the subject during the administration of President Adams, when the sudden augmentation of the navy rendered the employment of domestic copper in the equipment of ships, an object of political as well as pecuniary moment; and a mission was authorized to proceed to Lake Superior. Of the success of this mission, as it has not been communicated to the public, nothing can with certainty be stated; but from the inquiries which have been instituted during the recent expedition, it is rendered probable, that the actual state of our Indian relations at that period arrested the advance of the commissioners into the regions where the most valuable beds of copper were supposed to lie, and that the specimens transmitted to Government were procured through the instrumentality of some friendly Indians employed for that purpose.

Such are the lights which those who have preceded me in this inquiry have thrown upon the subject, all of which have operated in producing public belief in the existence of extensive copper mines upon Lake Superior, while travellers have generally argued that the southern shore of the lake is most metalliferous, and that the Ontonagon river may be considered as the seat of the principal mines. Mr. Gallatin in his report on the state of American manufactures in 1810, countenances the prevalent opinion, while it has been reiterated in some of our literary journals, and in the numerous ephemeral publications of the times, until the public expectation has been considerably raised in regard to them.

Under these circumstances the recent expedition under Gov. Cass entered the mouth of the Ontonagon river on the 27th of June, having coasted along the southern shore of the lake from the head of the river St. Mary, and after spending four days upon the banks of that stream in the examination of its mineralogy, proceeded on the first of July towards the Fond du Lac. While there, the principal part of our force was encamped at the mouth of the river, and the governor, accompanied only by such persons as were necessary in the exploration, proceeded in two light canoes to the large mass of copper which has already been described. We found the river broad, deep, and gentle for a distance, and serpentine in its course,—then becoming narrower, with an increased velocity of current, and before reaching the

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* Mackenzie's Voyages, p. 29.

copper rock, full of rapids and difficult of ascent. At the distance of three or four leagues from the lake, it is skirted on either side by a chain of hills whose extreme elevation above the bed of the Ontonagon may be estimated at from three to four hundred feet. These hills appear to be composed of a nucleus of granite, arising through a stratum of red sand-stone, and covered by a very heavy deposit of alluvial soil full of water-worn fragments of stones and pebbles, and imbedding occasional masses of native copper. Such is the character of the country in the immediate vicinity of the copper rock, and the latter is manifestly one of those imbedded substances, which has been fortuitously exposed to the powerful action of the river against an alluvial bank.

During our continuance upon this stream we found, or rather procured from the Indians, another mass of native copper weighing nine pounds (Troy) nearly; which will be forwarded to the War Department. This specimen is partially enveloped by a crust of green carbonate of copper, which is in some places *fibrous*, and on the under side mixed with a small portion of adhering sand, and some angular fragments of quartz, upon which it appears to have fallen in a liquid state. There is also an appearance of crystallization upon one side of it, and a portion of adhering black oxide, the nature of which it is difficult to determine by ocular inspection. Several smaller pieces, generally weighing less than a pound, were also procured during our excursion up the Ontonagon, and in the regions east of it; but all, excepting those cut from the large mass, are somewhat oxidated, or otherwise encrusted upon the surface. The geological structure of the country in detail, and the mineral appearances of the shore about the copper rock and at other points along the river, between that and the lake, are also of a highly interesting character, but do not appear to me to demand a more particular consideration in this report.

The discovery of masses of native copper is generally considered indicative of the existence of mines in the neighbourhood. The practised miner looks upon them as signs which point to larger bodies of the same metal in the earth, and is often determined, by discoveries of this nature, in the choice of the spot for commencing his labours. The predictions drawn from such evidence, are also more sanguine in proportion to the extent of the discovery. It is not, however, an unerring indication, and appears liable to many exceptions. A detached mass of copper is sometimes found at a great distance from any body of the metal, or its ores; and these, on the contrary, often occur in the earth, or imbedded in rock strata, where there has been no external discovery of metallic copper to indicate it. So far as the opinions of mineralogical writers can be collected on this point, they teach,—that large veins of native copper are seldom found, but that it is frequently disseminated in masses of various size in the rocks, and among the spars and ores of copper and other mines; and when found in scattered masses upon the surface, is rather to be considered as a token of the existence of the sulphuret, the carbonate, and other ores of copper, within the circle of country where it occurs, than as the precursor to contiguous bodies of the same metal. "Native copper," says Cleveland, "is found chiefly in primitive rocks, through which it is sometimes disseminated, or more frequently it enters into the composition of metallic veins, which traverse these rocks. It is thus connected with granite, gneiss, micaceous and argillaceous slates, granular limestone, chlorite, serpentine, porphyry, &c. It also occurs in transition and secondary rocks. It accompanies other ores of copper, as the red oxide, the carbonate and sulphuret of copper, pyritous and gray copper, also the red and brown oxides of iron, oxide of tin, &c. Its usual gangues are quartz, the fluate and carbonate of lime, and sulphate of barytes. At Oberstein it occurs in prehnite; and in the Faroe islands it accompanies zeolite.

"Native copper is not rare, nor is it found in sufficient quantity to be explored by itself. It sometimes occurs in loose, insulated masses of considerable size *."

From all the facts which I have been able to collect on Lake Superior, and after a deliberation upon them since my return, I have drawn the following conclusions:

1st. That the alluvial soil along the banks of the Ontonagon river, extending to its source, and embracing the contiguous region which gives origin to the Menomonie river of Green Bay, and to the Ousconsing, Chippeway and St. Croix rivers of the Mississippi, contains very frequent, and some most extraordinary imbedded masses of native copper; but that no body of it, which is sufficiently extensive to become the object of profitable mining operations, is to be found at any particular place. This conclusion is supported by the facts already adduced, and, so far as theoretical aids can be relied upon, by an application of those facts to the theories of mining. A further extent of country might have been embraced along the shore of Lake Superior, but the same remark appears applicable to it.

2d. That a mineralogical survey of the rock formations skirting the Ontonagon, including the district of country above alluded to, would result in the discovery of very valuable mines of the sulphuret, the carbonate and other profitable ores of copper; in the working of which the ordinary advantages of mining would

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* Cleveland's Mineralogy, p. 450.

be greatly enhanced by occasional masses and veins of native metal. This deduction is rendered probable by the general appearance of the country, and the concurrent discoveries of travellers,—by the green-coloured waters which issue in several places from the earth,—by the bodies of native copper found, —by the cupreous tinge which is presented in the crevices of rocks and loose stones,—by the geological character of the country, and by other analogous considerations.

These deductions embrace all I have to submit on the mineral geography of the country, so far as regards the copper mines. Other considerations arise from the facilities which that section of country may present for mining operations,—its adaptation to the purposes of agriculture,—the state and dispositions of the Indian tribes, and other topics, which a design to commence metallurgical operations at the present period would suggest. But I am not aware that any such views are entertained by Government, and have not considered it incumbent upon me in this communication to enter into details on these subjects. It may be proper, however, to remark, that the remote situation of the country containing the most valuable mines, does not, at the present period, favour the pursuit of mining. It would require the employment not only of the artificers and labourers necessary to conduct the working of mines, but also of a military force to protect their operations,—first, while engaged in exploring the country, and afterwards, in their regular labours. For, whatever may be their professions, the Indian tribes of the north possess strong natural jealousies, and, in situations so remote, are to be restrained from an indulgence in the most malignant passions, only by the fear of a prompt military chastisement. In looking upon the southern shore of Lake Superior, the period appears distant, when the advantages flowing from a military post upon that frontier will be produced by the ordinary progress of our settlement;—for it presents few enticements to the agriculturist. A considerable portion of the shore is rocky; and its alluvions are in general of too sandy and light a texture for profitable husbandry. With an elevation of six hundred and forty-one feet above the Atlantic Ocean *, and drawing its waters from territories all situated north of the forty-fourth degree of north latitude, Lake Superior cannot be represented as enjoying a climate very favourable to the productions of the vegetable kingdom. Its forest trees are chiefly those of the fir kind, mixed with white birch, (*Betula papyracea*, the bark of which is so much employed

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* This level is predicated upon the following facts and estimates which I extract from my "Narrative Journal."

"Elevation of Lake Erie above the tide waters of the Hudson according employed for canoes by the northern Indians,) and with some varieties of poplar, oak, and maple. The meteorological observations which I have made, indicate, however, a warm summer, the average heat of the month of June being 69°; but the climate is subject to a long and severe winter, and to storms, and sudden transitions of temperature, during the summer months. We saw no Indian corn among the savages upon this lake; whether the climate is unfavourable to its growth, or the wild rice (*Zezania aquatica*) furnishes an adequate substitute, is not certain. A country lacking the advantages of a fertile soil, may still become a very rich mining country, like the county of Cornwall in England, the Hartz mountains in Germany, and a portion of Missouri in our own country; but this deficiency must be compensated by the advantages of geographical position, contiguous, or redundant population, and the facilities of a ready commercial intercourse. To these, the mineral district of Lake Superior can advance but a feeble claim, while it lies upwards of three hundred miles beyond the utmost point of our settlements on the north-western frontier, and in the occupation of savage tribes whose hostility has been so recently manifested. Concerning the variety, importance, and extent of its mineral productions, little doubt can remain. Every fact which has been noticed tends to strengthen the belief, that there are extensive copper mines upon its shores, while the information that has been gathered in the course of the late mission, renders it certain that not only copper, but iron, lead, plumbago, and sulphur are productions of that region, together with several of the *precious siliceous** and *crystallized minerals*. It is rendered probable also, that silver ore is imbedded in the transition rocks of the region; and whenever it shall become an object with the American government, or people,

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	feet.
according to the Report of the New-York Canal Commissioners	560
Estimated fall of Detroit river, 20 miles at six inches per mile ..	10
St Clair river, 30 miles at four inches	10
Rapids of St. Clair river at the outlet of Lake Huron, in the distance of three miles	9
Estimated fall of the river St. Mary, between the Detour and Point Iroquois, 60 miles at three inches per mile (rapids not included)	15
Nibish Rapid	9
Sugar Island Rapid ... ..., .,.	6
Sault de St. Marie (according to Col. Gratiot) ..., ...	22·10
Level of Lake Superior	611·10

* The Carnelion is first found on approaching the Pictured Rocks on Lake Superior, and afterwards becomes very abundant along the shore extending to the Fond du Lac Sandy Lake on the head of the Mississippi, is a good locality of this mineral, and it is found around the shores of the numerous little

people, to institute mineralogical surveys of the country, no doubt can be entertained but such researches will eventuate in discoveries of a highly interesting character, and such as cannot fail both to augment our sources of profitable industry, and to promote our commercial independence. In the event of such operations, the facilities of a ready transportation, either in vessels or barges, of the crude ore to the Sault de St. Marie, will point out that place as uniting with a commanding geographical position, superior advantages for the reduction of the ores, and for the subsequent conversion of the metal either into ordnance or other articles. At this place a fall of twenty-two feet in the river in the distance of half a mile, creates a sufficient power to drive hydraulic works to any extent; while the surrounding country is such as to admit of an agricultural settlement.

I accompany this report with a geological chart of a vertical section of the left bank of the Mississippi at St. Peter's, embracing a formation of native copper, and in which the superposition of the layers of rock, and the several subdeposites forming the alluvial stratum, exhibit a remarkable order. The curvatures in the lines of the alluvial stratum, represent a natural mound or hillock recumbent upon the brink of the river, which has partially fallen in, thus exposing its internal structure. The formation was first noticed by the garrison who quarried stone for quicklime, and for the purpose of building chimneys, at this spot. The masses of copper found are all small, none exceeding a pound in weight. I have the honour to be, sir,

With great respect and regard,
Your most obedient servant,
(Copy.) HENRY R. SCHOOLCRAFT.

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little lakes in that region. In descending the Mississippi it is constantly met with in the alluvial soil. At the foot of the Falls of St. Anthony it is sparingly found; around the shores of Lake Sepin it is very abundant, and it may be traced below Prairie du Chien, and even as low as St Genevieve, as I have mentioned in my view of the mines. According to the classification of Werner, which is founded on "alternate bands of red and white," many of these specimens may be considered as Sardonyx. They are often associated with common chalcedony, with cacholong, and with certain varieties of agate and flinty jasper. In a few instances the common opal, in small fragments, is met with.

LXXV. *Observations and Experiments on the Rose of Jericho; with brief Notices of its History.* By JAMES MILLAR, *M.D., Fellow of the Royal College of Physicians and Lecturer on Natural History and Chemistry, Edinburgh.*

THE singular property which the dried specimens of the Rose of Jericho possess of expanding in water, has long attracted the attention of naturalists, and in times of ignorance has given rise to many superstitious notions. Some time ago I had an opportunity of examining two specimens of this plant, which have been deposited for several years among other rarities, natural and artificial, which adorn the elegant museum of the late Mr. John Thomson, merchant, in Edinburgh; and having been permitted through his liberality to make experiments on these specimens, I now state the result. The specimens alluded to were examined by many naturalists in Edinburgh, and by some strangers; both in their state of contraction and expansion; and, as far as I could learn from Mr. Thomson, none of them had ever seen any thing of the kind; so that such plants are rare in this country. Mr. Thomson received one of the specimens from a friend in Aberdeenshire, in whose possession it had long remained, but the precise time could not be ascertained; and he purchased the other in Holland, without knowing its peculiar property, and it lay unheeded in his cabinet for many years. The discovery was accidentally made by looking into the work of Le Brun, a French traveller, whose description will be afterwards noticed. The history of the specimens in Mr. Thomson's possession shows that the Rose of Jericho retains its singular property of expansion by the absorption of water, and of contraction when dried for a long course of years. One of the specimens it is pretty certain has been in a dry state for at least twenty years, and perhaps for a much longer time.

In the work of Le Brun now alluded to, and in which an account of his "Voyage in 1675 to the Levant, Egypt, and Syria," is detailed, (published in folio at Delft, in 1700,) I find the following observations on this plant: "Among other rarities," says he, "I purchased some Roses of Jericho, a very curious plant. In less enlightened periods of society, when mankind were more credulous, many stories were related of these roses. Among others it was asserted that, if they are put into water on Christmas eve they expand, which did not take place at any other time. This happened, it was said, as a memorial of the birth of Jesus Christ. But I know certainly," says the author, "that they have this property at all times, both night and day; and when they are taken from the water, they gradually contract." p. 301.

The same plant and its remarkable properties are spoken of by Caspar Bauhin (Pinx, p. 484.) The name *Rose of Jericho*, according to the account of it quoted from Bellonius, was first given by a monk, obviously in allusion to its supposed miraculous expansion at a certain season; for it is not a native of the country around Jericho, but of the sandy shores of Arabia Deserta, and a wild species (*sylvestris*) is found among the houses and waste places of Syria. Bauhin adds, that he had it in his garden; where it flowered for several years.

John Sturmius, a Professor of Louvain, who lived in the fifteenth or beginning of the sixteenth century, wrote a book on the Rose of Jericho, which is said to be full of superstitious details of its miraculous powers; and in allusion to the same superstitions it was sometimes called by others *Rosa Mariæ*.

In later botanical works the Rose of Jericho is distinguished by the generic name of *Anastatica*, derived from its property of reviving in water. Under this name it is described in *Hortus Cliffortianus*, p. 318; and as it approaches in some of its characters to the genus *Thlaspi*, it is arranged by Morison, Hist. Plant. ii. p. 228, under that genus, as *Thlaspi Rosa de Hiericho.*

The following characters are given by Willdenow:

ANASTATICA. Class and Order. *Tetradynamia Siliculosa.*

Gen. Char.—Silicula retusa, margine coronata valvulis dissepimento duplo longioribus; stylo intermedio mucronato obliquo, loculis 2-spermis.

Anastatica Hierochuntica. Foliis axillaribus brevissimis, siliculis ungulatis spinosis.

Native of the shores of the Red Sea, sandy places of Palestine and Barbary, and near Cairo in Egypt.

This species, which is an annual plant, was cultivated in Kew Garden in 1656, by the celebrated old botanist Tradescant. Another species, *Anast. syriaca*, is a native of Austria, Stiria, Carniola, Syria, and Sumatra.

It may be added that the appearance of the dried plant of the common Rose of Jericho, *Anastat. Hierochuntica*, indicates nothing of its resuscitating quality; for it is of a hard woody structure and consistency, somewhat resembling a plant of dried heath.

Experiments.—The weight of the specimens on which the experiments were made was first ascertained, and was found as follows:

Long-stemmed specimen	90 grains.
Short-stemmed ditto	110

Exp. 1.—Both specimens were immersed in water at the temperature of the air of the apartment about 50° Fahr. to within an inch of the division of the branches. After twelve hours no apparent change had taken place, excepting that numerous air-bubbles had collected on the stems, showing that air had been displaced by the absorption of water. Having remained in this state of immersion for forty-six hours without further appearance of change; the specimens being again weighed, gave the following results:

The long-stemmed specimen weighed	96 grains.
The short-stemmed ditto ditto	116

It is rather a singular coincidence, that in this case both specimens acquired exactly the same additional weight; namely, six grains, although the short-stemmed specimen, having a larger head, and being twenty grains heavier, might have been expected to imbibe a larger portion of water. But here it must be recollected that no expansion took place, and perhaps the absorption of the liquid was confined to the stems.

Exp. 2.—In a second experiment both specimens were again immersed in water, which rose so high as to cover the lower divisions of the ramifications. In five hours the expansion was complete. But when the level of the water was diminished by the absorption and evaporation, contraction of the branches immediately commenced; and when the vessels were again filled up, both specimens were restored to a state of full expansion. During the period of immersion, which was continued forty-two hours, the alternate expansion and contraction were several times repeated. At the conclusion of this period, when both specimens were fully expanded, they were weighed, and it was found that the augmentation of weight acquired by the short-stemmed specimen amounted to 37 grains, and therefore it weighed 147 grs. The weight gained by the long-stemmed specimen was equal to 28 grains, for it weighed 118 grains.—Fig. 1, Pl. V. is a view of one of the specimens in its contracted state, and fig. 2 is the same specimen in its state of expansion.

From the experiment now detailed, it appeared that the amount of absorption, or the increase of weight gained by each specimen, approaches very nearly to the ratio of their respective weights in the dried and contracted state. For 37, the additional weight of the heavier or short-stemmed specimen, is nearly in the same proportion to 110, the original weight, as 28 is to 90.

It might have been desirable to vary these experiments by immersing the specimens in water at different temperatures; but as they were considered valuable and curious on account of their resuscitating quality, it seemed unsafe to employ water much increased in temperature, in case that property should be diminished or destroyed.

In considering the resuscitating power of this plant by the absorption of water, is it to be supposed that any portion of the living principle still remains with it? A dried piece of wood absorbs water; but this is obviously dependent on the attraction between the fibres of the wood and the water; and no change that indicates the presence of a living agent appears. Some plants, and particularly of the tribe of Mosses, after being dried, when exposed to moisture or immersed in water, in a short time exhibit all their freshness and vigour.

I have only further to observe, that in the Rose of Jericho, the absorption does not take place at the extremity of the stem, as is proved by the first experiment, but at the commencement of the ramification, as appeared from the result of the second experiment; for the full expansion was not produced till the water rose to that point from which the branches pass off.

Edinburgh, May 1821.

LXXVI. *Con-*

LXXVI. *Concessions to Mr.* Ivory. *In a Letter to the Editor.*

Sir,—I must apologize to your readers for recalling their attention, after an interval of four or five months, to a question which they perhaps were rather pleased than sorry to think not likely to be revived. But I have some concessions to make to Mr. Ivory, and the computations of such a mathematician, as he is, are not to be hastily or lightly examined, even by a person as completely at his leisure, as the Coronation and the summer amusements of a watering place can have left him.

I have no pretensions to "*wit,*" and I am not fond of "bantering" or "sneering;" but I do claim the merit of preserving my temper unruffled, even when I am *attacked,* without provocation, on my own ground: when my assertions are not only denied, but even my "*veracity*" is called in question.

I am therefore most ready to admit, in the present instance, that the slight difference of the elements of the cohesive properties of mercury, employed by the English and French philosophers, appears to make a greater difference in some of the results of the calculation, than I was at all aware, before I read Mr. Ivory's last paper. I will also admit that Mr. Ivory has convinced me, *by means of the series,* that his formulæ are more accurate than I had some reason to suppose: for it was only from such a comparison, that I could form any opinion, how far the quantities, which he has professedly neglected, were or were not wholly inconsiderable. And this is still my principal objection to his "refinements."

On the other hand, the series, "*clumsy*" as it certainly is, does afford in all cases a mode of *estimating* the utmost possible magnitude of the terms omitted: and if we may trust Mr. Ivory's own latest calculations, it agrees, even in the last place of decimals employed, with the formulas which he supposes to be sufficiently correct. When however he "took the series into his hands," I really was in hopes that he would have employed his undoubted talents in improving it, or in facilitating its application: and that the conclusions, which he considers as *probably* deducible from it, would at least have been converted into *certainties.* The labour of a few days would be sufficient to compute coefficients enough to remove much of the difficulty; and when once computed, they would last for ever. But it is said that great mathematicians have often been bad computers.

I feel no reluctance in admitting that the table of 1809 is imperfect, not only in the instance which Mr. Ivory has re-computed very accurately, by the help of the *clumsy* series (p. 425); but perhaps in many others. It is however remarkable enough, that this table, with all its imperfections, is, *in every instance,* nearer to Mr. Ivory's own, than that of Laplace, which he says has been "sneered at" in the article COHESION. The author of that article does indeed observe, that Mr. Laplace has had recourse to "the awkward contrivance of building up a curve, like the arch of a bridge, with fourteen blocks on each side:" the same contrivance having been originally employed in this country, and then abandoned for the series: and Mr. Ivory's own table appears fully to justify this abandonment: nor is it easy to conceive why he persists in dragging forwards the most illustrious of the French mathematicians into a comparison, which, with respect to the present investigation, cannot but be highly disadvantageous to him.

If, indeed, *the truth is to be told*, Mr. Laplace's whole physical theory of capillary action is rendered NUGATORY and DELUSIVE, by the omission of exactly one half of the conditions of the problem. He has attempted to deduce the laws of the equilibrium of two forces from the determination of the magnitude of one of them only. For it is most manifest and undeniable, that no substance, subjected to the operation of an attractive force, can remain at rest, without having that attraction counteracted by *an equal repulsive force:* and it is equally obvious that, in all common cases, the general amount of attraction and repulsion, reduced to any given direction, with regard to any given atom, must be equal. If therefore we suppose the joint or remaining force, depending on the mutual actions of two particles only, to be represented by a certain function of their distance, it is obvious that this function must be such as to afford a sum or integral, for all the particles within the sphere of corpuscular action, $=0$. And this is the true reason why Mr. Laplace's earliest computations have been silently abandoned, as affording no practical result; and why they never can be resumed, even by those whom they have dazzled and astonished. I am, Sir,

your very obedient servant,

London, 3 Nov. 1821. S. B. L.

POSTSCRIPT. Though I am perfectly disposed to remain at peace with Mr. Ivory, I am obliged to add some further conditions to my capitulation; since he does not appear to have adverted to my last postscript: otherwise he could scarcely have remarked that I admit the number ·00418, and that I ought to have said ·00419. This assertion brings the discussion within very narrow limits: I have chosen the case in which the two methods appeared to differ the most widely, and which is one of the most unfavourable to the convergence of the series: and one case is as good for the present purpose as a thousand. The difference of the results is only $\frac{1}{140}$th of the whole quantity, and its linear amount is no more than the thirty thousandth of an inch. But in order to examine whether this error belongs to the original series, or to Mr. Ivory's supposed improvement, I shall now follow the steps of my last postscript with still smaller portions of the curve, computing the value of s for $x=\cdot25$, $x=\cdot28$, and $x=\cdot30$, in succession. We shall then have, in the first place, when b is $\cdot4160$, for $x=\cdot25$, $s=\cdot38086$, $u=\cdot92463$, $t=\cdot41201$, and $y=032756$, as before. In the second place, taking $\Delta x=\cdot03$ instead of $\cdot05$, we easily obtain, from the former computation, $\Delta s=\cdot15083+\cdot03077+\cdot00500+\cdot00085+[\cdot00017]$, and $s=\cdot56848$: the angle being about 34° 39′, the cosine $u=\cdot82270$, and the tangent $t=\cdot69100$. The new value of y may be obtained, with sufficient accuracy, from the original series, which is more convergent for the ordinate than for the sine, and it will be found $y=\cdot04830$. With these values we proceed to find $A=7\cdot630$, $B=118\cdot2$, and $C=2461\ 2$, which are sufficient to make $s=\cdot5685+\cdot1526+\cdot0236+\cdot0066+[\cdot0022]=\cdot7513+[\cdot0022]$: so that this computation fully confirms the suspicion expressed at the end of the postscript, that the depression $\cdot004160$, instead of being too small, is *somewhat too great*, for a tube six tenths of an inch in diameter.

Until therefore Mr. Ivory shall condescend to point out some error, either essential or accidental, in this computation, I must still be allowed to assert, *first*, That the series, with the assistance of the Taylorian theorem, or of Taylor's theorem, by all means, if Mr. Ivory likes the name better, though somewhat inconvenient for calculation, is still both true and sufficient: and *secondly*, That Mr. Ivory's approximations, professedly leaving out some small quantities as inconsiderable, are unsatisfactory, because they afford no ready means of appreciating the utmost possible value of the quantities so neglected: and because it appears from these computations, that their very able author himself has in fact much underrated the importance of these quantities.

On the other hand I must admit, that the accuracy of the series, in its original form, appears to me to have been somewhat too highly appreciated by the author of the article COHESION: and that if the further prosecution of the inquiry were of any material importance, it would be right to employ a professional computer to enlarge the number of the co efficients, unless indeed Mr. Ivory's ingenuity and experience could point out some less laborious method of determining them, than that which arises from the direct solution of the problem, by the method hitherto employed.

LXXVII. *The*

LXXVII. *The second Portion of a Catalogue of* 1800 *zodiacal Stars, for the Epoch of January* 1, 1800; *from the Works of* Herschel, Piazzi, Bode, *and others, with illustrative Notes. Selected and arranged by a Member of the Astronomical Society of London.*

Constellations: *Taurus, Auriga, Orion, Gemini.*

Synonyms.			Character	Constellation.	Mag.	IV hours. Right Asc.			Declination. +		Lat
P.	B.	F C M				m.	° ′ ″	A V.+	° ′ ″	A V +	°
6	192	110		Tau.	7	1	60 15 14 4	53·01*	21 53 13·5	9·94*	1·3
16				—	8	3	45 18 6	49 26*	10 30 27 0	79*	—10 0
21	202	48		—	6	4	61 6 27·0	50 76	14 53 22 2	79	— 5·8
27	206	50	ω 2	—†	5 6	6	23 21 6	52 36	20 4 27·5	·61	— 0·8
32	207	51		—	7	7	38 30 0	52 93	21 4 41 0	·52	0 1
36	208	53		—	6 7	8	54 55 2	52 64	20 38 45·5	·42	— 0 3
37	215	56		—†	6 7	8	56 50 4	52 90	21 16 40 5	38	0 2
38	209	52	φ	—†	6	8	62 1 12 6	54 94	26 51 32 5	·35	5 8
39	210	54	γ	—†	3·4	8	6 22 8	50 94	15 7 57 4	·37	— 5 8
40	211	55		—	7 8	8	7 11 2	51 18	16 1 43 4	37*	— 4·8
41	213	57	(h.1)	—†	6	9	10 44 1	50·45	13 32 32 0	45	— 7 3
42				—	8	9	14 35 2	54 99*	26 51 29·7	33*	5 8
43	216	58	h.(2)	—	6	9	19 5 2	50 63	14 36 17 0	·36	— 6 3
45	218	M. 142		—†	6 7	10	21 38 4	50 42	13 22 30 0	30	— 7 4
47	219			—	7 8	10	26 3 4	52 68*	20 33 8 9	·28*	— 0 3
48	220			—	7 8	10	27 7 4	52 73*	20 41 53 0	·27*	— 0 2
51	223	59	χ	—†	6	10	36 26 4	51 37	25 8 41 0	·21	4 0
53	224			—	7	11	39 27 0	52 62*	20 20 9 4	·20*	— 0 6
51	222	60	(h.3)	—†	7	11	41 58 5	50 35	13 35 35 2	·24	— 7·4
57	226	61	δ.1	—†	4	11	51 13 2	51 54	17 3 45 6	·22	— 4 0
61	227			—	7 8	12	56 33·0	52 69*	20 30 10 7	·12*	— 0 5
62	228	63		—	6	12	59 20 8	51 33	16 17 56 5	16	— 4 7
63	229	62		—†	7	12	59 22 5	53·98	23 49 21 0	09	2 6
64	231	64	δ.2	—	4 5	13	63 8 44 1	51 62	16 58 7 8	·09	— 4·1
69	221			Pers.	6 7	13	20 58 5	56 76*	30 58 25 7	00*	9·8
	234			Tau. †	6	13	20 14·	52·0*	18 34 31	0*	— 2·5
70	235	65	κ.1	—	5 6	13	22 0 0	53·18	21 49 25 0	8 94	0·6
71	236	67	κ 2	—	6 7	14	22 49 5	53 27	21 43 48 0	96	0·5
73	237	68	δ.3	—†	5	14	28 55 9	51 71	17 27 29 0	·98	— 3 7
74	238	70		—	7	14	33 16 5	51 03	15 28 19 5	9 01	— 5·7
75	239	69	υ.1	—	5	14	35 18 0	53 43	22 20 49·7	8·89	1·1
76				—	8	14	37 1·5	52 91*	21 0 13 1	·91*	— 0 1
78	240	71		—	5 6	15	44 22·5	50·84	15 9 8 0	·68	— 6 0
79	242	73	π	—	5	15	49 49 2	50·56	14 14 58·4	·82	— 6 9
80	243	72	υ.2	—	6	15	50 12 0	53·47	22 31 57 2	·84	1 2
	-			—†	9	15	50 3		18 39·6		— 2·7
				—†	8	16	52 8		17 44·6		— 3·3
82	247			—	7·8	16	64 2 21·0	52·99*	21 9 40 4	·77*	— 0 1
86				—	9	17	13 52·8	51·14*	15 50 33 0	·71*	— 5 4
87	249	74	ε.	—†	4	17	14 17·1	52·23	18 43 27·5	·72	— 2 6

Synonyms.			Character.	Constellation	Mag.	IV hours. Right Asc.					Declination. +				Lat.
P.	B.	F.C.M				m.	°	′	″	A.V +	°	′	″	A.V. +	°
88	251	75		Tau.	6	17	64	15	21·0	51·22	15	54	0·7	8 75	—5 4
89	252	76		——	7	17		16	0·3	50·63	14	17	0·6	·66	—7·0
90	254	77	9.1	——†	5	17		17	27·6	50 98	15	30	23·5	·76	—5 8
91	256	78	9.2	——†	5·6	17		18	52·2	51·16	15	24	57·0	·79	—5 9
93	·257	79	♄	——	6	18		24	38·2	50 16	12	35	32·0	·65	—8 7
95	260	M. 158		——	7·8	19		38	43·5	52 39*	19	23	29 3	·58*	—2·0
97	261	80		——†	6	19		41	17·7	50·99	15	11	20·5	·65	—6 2
99	266	M. 160		——†	5·6	19		46	56·5	51·13*	15	44	49·3	·54*	—5·6
100	267	. 81		——	5·6	19		48	46 9	51·10	15	14	42 0	·57	—6·1
102	269	M. 162		——	8	19		50	12·3	51·12*	15	42	9·6	·52*	—5 7
103	268	83		——	6	19		50	37·5	50·37	13	16	40 0	·54	—8·1
105	271	84		——	7	20		56	43·5	50 77	14	39	42·0	48	—6 7
106				——	9	20		58	9 6	50·63*	14	14	39·7	·48*	—7·1
108	273	85		——	6	20	65	6	46 5	51 03	15	24	38·5	·49	—6·0
111	1		(t)	Aur.	7	22		32	1·5	55 93*	28	31	47 2	·30*	7·0
113	275	M. 163		Tau.	7 8	22		33	5 1	51·19	15	53	21·4	·30*	—5 6
114	276	86	ε	——	5	22		37	38 1	50 84	14	24	45·7	·35	—7 2
116	277			——	7 8	23		38	16 5	50 17*	12	49	8 8	·27*	—8 5
119				——	8	24		59	11·5	52 52*	19	32	40·0	16*	—2 1
120	282	M. 166		——	8	24		59	56 7	52 49*	19	27	25·0	·16*	—2 2
125	283	87	α	——†	1	24	66	6	51·3	51 34	16	5	42 0	·02	—5·5
				——†	7	26		32	1		26	30	8		4·9
135	287	89		——	7	27		40	49 2	51 23	15	37	12 0	7 97	—6 0
138	288	90	c.1	——†	5	27		41	51 3	49 96*	12	5	50 7	89	—9·5
143	291	91	σ.1	——	5·6	28		56	12 7	51 13	15	23	36 0	81	—6 3
145	292	92	σ.2	——	5·6	28		57	45 9	51 31	15	30	32 5	87	—6 2
148	5	-	(u)*	Aur.	6 7	29	67	12	26·2	55 92*	28	12	49 5	76*	6 5
149	294	93	c 2	Tau.	5	29		13	57 7	49 89	11	47	33 5	76	—9 9
159	295	94	τ	——†	5	30		33	51 0	53 73	22	33	37 0	·67	0·7
162	298	95		——†	7	31		47	0 0	54 11	23	41	48 5	·61	1 7
163	296			——	8	31		47	56·2	52 16*	18	19	51 0	·57*	—3 6
168	7			Aur.	· 8	33	68	19	45 9	56 03*	28	16	48 2	40*	6·4
177				Tau.	8	34		36	30 9	52 23*	18	25	17·5	31*	—3 5
179	302	M. 172		——	6	35		39	13 2	52·21*	18	21	37 7	29*	—3 6
185	11			Aur.	7	36	69	6	0·0	57·81*	32	13	19 6	15*	10 2
190	305	M. 173		Tau	8	37		15	2 7	52 24*	18	21	47 0	10*	—3 7
194				——	7·8	38		31	15·0	51·23*	15	31	36·5	·01*	—6 5
195	306	96	(K)	——	6	38		34	30·6	51·17	15	32	33·0	·00*	—6 5
208	307	97	i	——	5 6	40		55	15 4	52·32	18	29	13 3	6·91	—3·7
211	17			Aur.	7	40	70	4	33·4	55 84*	27	32	56·7	·83*	5 4
216	12	4	o.1	Ori.	5	41		18	24·0	50·61	13	54	17·5	·69	—8·3
222	311	M. 176		Tau.	8	43		37	35·2	51·68*	16	41	5 0	·64*	—5·5
228	313	M. 177		——	8	43		51	31·8	51·46*	16	2	55 4	·57*	—6 2
231	314	M. 178		——	8	44		57	24·7	51·55*	16	17	5 7	·54*	—6 0
240	22	9	o.2	Ori.†	5	45	71	17	0 0	50 34	13	11	11 0	·44	—9·1
243	317	M. 179		Tau.	6·7	46		25	20·4	54·32*	23	37	21·8	·39*	1·4
246	318	M. 180	(1)	——†	6·7	46		27	30·0	51·76*	16	49	37·4	·37*	—5·5
247	319	98	k	——	6	46		28	51·4	54·67	24	43	39·5	·30	2 3
257	26	M. 181		Ori. †	7	48		54	50·2	50·84*	14	13	28·4	·22*	—8·1
261	322	101		Tau.†	7	48	72	4	2·2	51·46	15	36	10·5	·24	—6·8

Synonyms.			Character.	Constellation.	Mag.	IV hours. Right Asc.					Declination +				Lat.
P.	B.	F.C.M.				m.	°	'	"	A.V.+	°	'	"	A.V.+	°
266				Ori.	7·8	49	72	18	23·2	50·80*	14	4	8·0	6·09*	—8·3
274	325	102		Tau.	4·5	51		47	15·9	53·52	21	17	27·5	5·91	—1·2
282	330	M. 184		——	7	52	73	6	48·0	53·36*	20	59	1·0	·82*	—1·6
286	34	11	y.1	Ori.	5	53		17	13·5	51·20	15	6	46·0	·78	—7·4
287	332			Tau.	8	53		22	55·5	55·44*	26	8	28·2	·73*	3·7
288	333	M 186		——	7 8	54		26	19·2	52 83*	19	31	5·1	·71*	—3·1
293	335	104	m.	——†	5	56		54	36·7	53·13	18	21	48·7	·63	—4·3
295	334	c. 152		——†	6	56		59	1 8	54·61	23	59	12·5	·53*	1·7
296	337	106	.1	——	5·6	56		59	40 5	53 02	20	8	28·0	·52	—2·5
297	336	105		——†	6	56		59	43 2	53 69	21	25	36 0	·57	—1·2
298				——	7	56	74	0	32 2	56 25*	27	59	45·0	·52*	5·4
305	339	107	1.2	——	7	57		15	43 2	52·92	19	35	11·2	·48	—3·1
				——†	8	57		18·1			27	45·7			5·2
313	51	15	y.2	Ori.	5	58		33	55 2	51·33	15	19	45·8	·41	—7·3
318	54			——	7 8	59		43	47·2	50 58*	13	17	2 6	·27*	—9·4
319	342			Tau.	8 9	59		43	53 8	53 17*	20	18	17·5	·27*	—2·3
1	58	c. 156	(y.3)	Ori. †	6 7	v	75	3	10 5	51 49*	15	47	9 2	·17*	—6·9
9	56	14	(a)	Aur.†	5	2		36	0 0	58 46	32	26	28·0	·02	9·6
13	346	108		Tau.	7	3		51	40 5	53 80	22	2	31 5	4 90	—0·8
				Aur.†	8	4	76	0 3			33	5 5			10·2
25				Tau.	8	6		25	24·0	53 07*	19	54	12·1	·71*	—2·9
34	350	109	u	——	5 6	7		48	55 5	53·78	21	52	30·5	·67	—1·0
37	351	M. 194		——	7	7		51	21 3	53 09*	19	54	40 0	·56*	—3·0
38				Ori	8	8		54	5·4	50 65*	13	20	33 6	·55*	—9·5
41	354	M. 197		Tau.†	7 8	8	77	6	38 4	56 29*	27	44	25 5	·48*	4 8
42	73			Aur.	6 7	9		7	31 5	57 00*	29	21	9·0	·47*	6·5
43	352	M 195?		Tau.†	7	9		7	45·4	52·88*	19	21	33·5	·47*	—3 6
46				Ori.	8	9		11	39·9	50 65*	13	19	49 8	·45*	—9 6
48	353	M. 196		Tau.	7 8	9		17	3 0	52 98*	19	35	52·3	·42*	—3·3
53				Aur.	9	10		36	15 0	56·50*	28	15	42·4	·29*	5·3
55	75	22		——†	7	11		40	50 5	56 76	28	43	52·2	·28	5·7
62	78			——	7	12		56	23 4	57 80*	31	1	21·7	·19*	8·1
63				——	7	12		56	30 0	57 76*	30	56	29·6	·19*	8 0
64	355	110		Tau.	7	12	78	1	15 6	51 75	16	29	49 0	·18	—6·5
66	356	111		——†	6	13		11	19 5	52 25	17	11	2·0	·12	—5·8
72	358	112	β	——†	2	14		24	51 9	56 74	28	25	25·5	3 83	5 4
	359	113		——†	6	15		38	5 9	51·84*	16	30	31·3	·95*	—6·5
86	360	115		——†	5 6	15		52	38 4	52 29	17	46	34·3	·86	—5·3
88	361	114	o	——†	5	16		54	26 5	53 90	21	45	9 5	·92	—1·3
89				——	8	16	79	0	1·5	51·60*	15	51	21·6	·82*	—7·2
90	362	116		——	6	16		4	5·1	51·54	15	41	33 0	·85	—7·4
92	363	117		——†	6	16		6	21·3	52 06*	17	3	34·8	·79*	—6·0
				——†	7	17		9	28·	51·75*	16	15	38·	·77*	—6·8
98	364	118		——†	7	17		14	35·4	55·25	24	58	25·0	·70	1·9
99	95			Aur.	7·8	17		14	43·9	56 93*	29	0	42·6	·74*	6·1
100				Tau.	8	17		16	15·1	53·29*	20	15	54·9	·73*	—2·7
				Aur.†	7·8	17		20·3			29	21·9			6·4
106	366			Tau.	8	18		35	33·7	53 34*	20	22	47·1	·63*	—2·6
107	367			——	7·8	19		39	51·0	54·13*	22	17	34·0	·60*	—0·7
114	100	25	χ	Aur.	5	20		55	45·7	58·44	32	1	42 0	·49	8·9

Synonyms.			Character.	Constellation.	Mag.	V hours. Right Asc.					Declination. +				Lat.
P.	B.	F.C.M				m.	°	′	″	A V.+	°	′	″	A V.+	°
115				Tau.	8	20	79	59	44·4	55·99*	26	49	9·3	3·48*	3·8
118	102			Aur.	7·8	20	80	3	31 5	58·41*	32	7	49·0	·46*	9·1
119	368	119		Tau.	5·6	20		7	21 9	52·57	18	25	54·7	·45	—4 7
125	372	M. 205		——	6·7	22		26	31·5	53 34*	20	19	6·8	·33*	—2·9
127	373	120		——†	6	22		27	7 6	52 60	18	23	3·0	·42	—4 8
131				——	8	22		37	19 9	56 01*	26	49	34 0	·27*	3·8
132	125	35	(u)	Ori.†	7	23		38	4·5	50 95	14	9	8·5	·29	—9 0
135	377	121		Tau.	6	23		48	38 2	54 80	23	53	31·5	·15	0 7
136	107			Aur.	6 7	23		50	44·4	56 32*	27	31	5·3	·19*	4·5
				Tau.†	8	24	81	6 3			21	51 4			—1 3
145				——	7	25		9	55 0	56 00*	26	47	4 9	·38*	3 7
148	382	122		——	6	25		21	53 1	52 07	16	54	10 5	2·99	—6 3
152	383	123	ζ	——	3·4	26		25	22 8	53 55	21	0	25 0	3·00	—2·2
155	115	26	(1)	Aur.†	5	26		27	0 0	57 56	30	21	34·7	·08	7 1
164		124?		Tau.†	8·9	27		46	34·5	54 51*	23	11	38·9	2 87*	—0 1
165	387	125		——	6	27		50	7 5	55·54	25	46	14 8	·88	2 5
168				Aur.	8	28		57	55 0	58·79*	32	46	20·6	·80	9 7
180	390	126		Tau.†	5 6	30	82	26	1 5	51 81	16	25	1·5	·68	—6·9
184				——†	7 8	30		29	42 0	54·67*	22	32	45·2	60*	—0·6
189				——	8	31		40	42 0	52·81*	18	52	36·3	·55*	—4 4
191	393	127		——	8	31		46	57 3	52·74	18	52	12 0	·52	—4·4
192	394			——†	8	31		47	47 2	54 49*	23	5	42 2	·52*	—0 1
198				——	8	33	83	7	55 5	52·76*	18	43	48 0	40*	—4 2
201	395	128	(M)	——†	6	33		20	25·2	51·58	15	59	11 0	·42	—7 3
202	396	M. 214		——	7 8	33		21	48 0	52 72*	18	36	13 0	31*	—4·7
210	397	M. 215		——	8	35		45	57 0	53 34*	20	11	25 0	18*	—3 2
212	398	129		——	6	35		48	51 0	51 61	15	43	53 5	·25	—7 6
214	399	M. 216		——†	7 8	36		54	51 0	55·13*	24	35	56 3	·13*	1 2
215	400	130	(N)	——	6	36		56	42 0	52 34	17	38	27·3	17	—5·7
216	401	131	(O)	——	6	36		57	30 4	51 22	14	24	4 0	·05	—9 0
218				——	9	36	84	0	51 6	51·59*	15	38	2 0	·09*	—7 7
221	402	133		——†	6	36		5	36 6	50 97	13	48	51 0	11	—9 6
222	191	M. 218		Ori.†	7 8	36		6	31 0	53·58*	20	47	7 5	·06*	—2·5
223	403	132	(B.	Tau.	5	37		11	6 3	55 03	24	29	10 0	·06	1 1
225				Aur.†	8	37		14	1·0	58·33*	31	42	22 9	·02*	8 5
236	406	c. 169		Tau.	7	38		35	32·2	56 37*	27	53	31 2	1 89*	4 6
237	142			Aur.	7 8	38		35	46 5	58 51*	32	3	6 5	·89*	8 9
240	407	135		Tau.	6	39		46	30 6	50 99	14	14	6 0	91	—9 2
242				——†	7	39		51	24 0	51 13*	14	22	20 9	80*	—9 1
241		138?		——†	7	39		52	19 2	50 98*	13	58	32 4	·79*	—9·5
247	409	136	(C)	——	4 5	41	85	11	24 0	56 43	27	33	0·4	·60	4 2
249	410	137	(D)	——†	6	41		15	11·2	51 05	14	6	27 0	·66	—9 3
251	203	M. 223		Ori.†	7 8	41		21	28 0	53 38*	20	14	19 7	·62*	—3·1
256	149			Aur.	6 7	42		30	6 0	58 33*	31	39	20 0	·57*	8·4
258				Ori.	9	42		35	1·5	50·94*	13	50	0 5	·54*	—9·5
259	210	54	χ·1	——†	5	43		38	6 6	53·15	20	13	31·2	·44	—3 2
265	215	57	χ·2	——†	6	43		46	39·3	53·29	19	41	50·5	·56	—3·7
266	154			Aur.	7·8	44		57	57·1	57 05*	28	53	42·3	·41*	5·7
272				Ori.	8	45	86	20	6·7	52 96	19	10	31 6	·28*	—4·2
273	413	139		Tau.	5·6	46		23	47·4	55·75	25	54	51·5	·25	2 5

Synonyms.			Character	Constellation.	Mag.	V hours. Right Asc.			Declination. +		Lat.
P.	B.	F.C.M.				m.	° ′ ″	A.V.+	° ′ ″	A V.+	°
279				Tau.	8	47	86 37 57·0	56·44*	27 31 39 0	1 18*	4 2
285	414	140	(Q.1)	——	8	48	87 5 12 0	54·46*	22 52 25·0	·02*	—0·6
287	415		(S)	——	7	48	·6 36 7	56·45*	27 32 49·8	·01	4 2
				——†	7·8	49	14 6		23 16·8		—0 2
296	416	141	(Q.2)	——	6	50	24 14 4	54·21	22 22 54 0	0·95	—1·1
300				Ori. †	8·9	51	49 21·6	52·39*	17 39 6·0	·76	—5·8
304	242	64	χ·4	——†	5·6	52	54 22 2	53·18*	19 40 49 5	·78	—3·8
306	7			Gem.	7 8	52	57 31 0	55 54*	25 26 12 8	·71*	2 0
307	8	1	h	——†	5	52	59 27·0	54·61	23 15 35·8	·63	—0·2
308	245	62	χ·3	Ori. †	5	52	88 0 40 5	53·42	20 7 44 7	·69	—3 3
317				——	8	54	25 32·7	51·55*	15 26 49·2	·55*	—8·0
323	10	2		Gem.	6·7	55	39 16 5	54·78	23 38 38·0	·57	0·2
325				——	8	55	42 46 5	56 08*	26 41 16 0	·45*	3 2
328	256			Ori. †	7	55	48 12 0	51 59*	15 33 8 5	·42*	—7·9
329	12	M. 234		Gem. †	7 8	56	55 1 5	54 39*	22 42 57 5	·38*	—0·8
332	257	67	ν	Ori.	4 5	56	89 2 15 0	51 30	11 46 47·0	·40	—8·7
338	14	M. 235		Gem.	6 7	57	22 16 5	54 19*	22 12 27 8	·22*	—1·3
340	15	3		——	6	58	23 47 4	54·58	23 7 55 0	·18	—0 4
341	16	4		——†	7	58	35 25 5	54 48	23 1 12·6	.15	—0·5
				Ori. †	7 8	59	41 3		15 55 9		—7 5
350	17	5		Gem.	7	59	49 3·6	55·15	24 26 59 5	·02	1·0

NOTES to the first Portion, *continued.*

P. II. 140, or *B.* 421 Ceti.) Is C.H. 195.

38 Arietis.). Is the same as 88 Ceti.

87 μ Ceti.) Proper motion in R.A. + 0″·20 per annum; in Declin. +0″·03.

42 π Arietis.) Treble. Hers. I. 64. "Excessively unequal; L. w; S. both mere points. With 227, neither of the small stars can be seen, except by continued attention; with 460, the nearest is 1½ or 1¾ diam. of L. The third is about 25″ or 26″ distant from L. Pos. of both, being all three in a line 19°·3 s. following."

46 ρ. 3 Arietis.) The annual pr. mot. by Fl. Br. and Piazzi is, R.A. +0″·34. Declin. —0″·17.

P. II. 215, or *B.* 475 Ceti.) Is C. H. 113. Br. has 2 obs. of the R.A. one of the Declination.

47 Arietis.) Double acc. to Piazzi, the other star 8 mag., about 2′ north.

Anon. R.A. 41° 52′.) R.A. by 2 obs. of Bradley. *Hist.Céleste,* p. 35.

50 Arietis.) Fl. R.A. requires —15′. It is 70 of Lacaille's Zod. Cat. though Pi. erroneously calls it 75.

3 A 2 *Anon.*

Anon. R.A. 44° 38′.) *Hist. Céleste*, p. 200. The R.A. obs. by Bradley.

B. 157 Arietis.) This is probably Herschel's double star V. 117. "About 1¼° n. preceding ζ Arietis, towards 41; the following of 4 forming an arch. Very unequal. Both dr. Distance 34″·8. Pos. 47°·55 n. preceding.

57 δ Arietis.) Pr. mot. in R.A. +0″·20.

P. III. 4, or *B.* 3 Tauri.) The Rt. Ascension in Bode's Catalogue is −4′ from Messier.

Anon. R.A. 47° 14′.) This is supposed to be Herschel's double star II. 76. Wollaston suggests that c. 75 may be the star meant, but that is only half a degree from the determining star. Pos. from *Hist. Céleste*, p. 33. "About 1° s. preceding 63 Arietis, towards μ Ceti; the most south of two small telescopic stars. Nearly equal. Both w. With 227, above 3 diameters; by the micrometer 5″·8. Pos. 15°·4 s. preceding."

63 τ. 2 Arietis.) Near this a double star. Hers. IV. 89. "The vertex of an isosceles triangle following τ Arietis; a very small star. Very unequal. L. r; S. d. Distance with 278, 20″·05. Pos. 62°·0 s. following." It is to be observed, that Herschel mentions only one τ Arietis, whereas 61 and 63 are both marked with that letter in the Brit. Cat. The uncertainty may be removed by referring to Sir William's description of his double star II. 76; where he distinctly assigns the letter in question to 63 only. Probably Piazzi's star 46, whose R.A. is +8′ 8″·4, and Decl. +13′ 50″·3, may be one of the stars of the isosceles triangle above mentioned. The proper motion of 63 Arietis in R.A. is −0″·18.

Anon. R.A. 48° 11′.) Lalande *Hist. Céleste*, 139, 201. This may perhaps be Herschel's star III. 91, although the distance from the determining star is rather greater than stated by him. "Double. Near 1° n. foll. 62 Arietis, towards ε Persei. Nearly equal. Both d.w. Distance 11″·3, not very accurate. Position 12°·4 n. prec. or s. following."

1 ο Tauri.) Double, according to Piazzi.

Anon. R.A. 49° 6′.) This is probably one of the two stars mentioned by Herschel, III. 77. "Double. About ¾° south following 65 Arietis, in a line parallel to the Pleiades, and ε Tauri; *the preceding of two.* Very unequal. L. r.; S. bluish. Distance 8″·53. Position 73°·3 s. following." There is only one star noticed by Lalande *Hist. Cél.* p. 33.

4 s Tauri.) A star of the 7th mag. precedes this 1^{m} 30^{s}. Decl. +2′. Also a double star. Hers. IV. 44. "A small telescopic star south following s Tauri. Extremely unequal. L. w. S. d."

7 Tauri.) Double. Hers. IV. 88. "Very unequal. L. pr.; S. dr. Distance 19″·83. Pos. 23°·25 n. following."

M. 113.) Mayer's R.A. is +23″·5, and Decl. +22″·6, but his is a single obs. and marked doubtful.

9 Tauri.) This star was observed twice by Flamsteed, and several times by Bradley (whose observations ascertained the position here laid down); also twice observed by Lalande, 28th Sept. and 28th Oct. 1793, as of the 7th mag. Yet Bode and Herschel could not see it, neither is it contained in Piazzi's Catalogue. It may therefore be considered variable.

Anon. R.A. 51° 29′.) *Hist. Cél.*, p. 195. Supposed to be Herschel's star III. 78. "Double. About 1¼° s. prec. 13 Tauri in a line parallel to ε Tauri and δ Ceti. Nearly equal. Both pr. Distance 7″·17. Position 87°·95 n. preceding."

C. 87.) In Wollaston's Cat. this star is set down as 87 Mayer. Very near it was the planet Ceres when first discovered by Piazzi, 1st Jan. 1801.

B. 34 Tauri.) A double star of the third class. Herschel MS. Jan. 1785.

Anon. R.A. 52° 47′.) *Hist. Céleste*, p. 200. Supposed to be Herschel's double star III. 88. "About ½° north following 11 Tauri, towards ι Aurigæ. Very unequal. L. w; S. pr. Distance with 278, 13″·6. Pos. 89°·85 n. foll."

THE PLEIADES.) In Piazzi's Catalogue are 28 stars distinguished as belonging to this celebrated group; the whole of them are inserted in the present collection, with an additional one from Bradley. Besides these, Bode's Catalogue comprises about 65 more, inserted from the observations of Lemonnier and Jeaurat; but they do not possess sufficient accuracy, either of position or magnitude, to justify the introduction of them upon the present occasion. Francis Baily, Esq. (to whose assistance the present writer acknowledges himself greatly indebted) has noticed that Jeaurat's stars are incorrectly brought up to the epoch of Bode's Catalogue, as the Right Ascensions ase all too little by 21″; and the Declinations too great by a quantity varying from 0″ to 20″, and depending upon the R.A. of each star.

The order of brightness among the stars of the Pleiades, comprised in the Brit. Cat. is given by Herschel as follows: 25—, 27. 17. 20, 19. 23—, 28, 18. 16 21, 22, 26, 24. Piazzi's magnitudes agree in general with this arrangement, with the exception of those assigned to 17, 28, and 16, which are estimated too high, as will be hereafter noticed. Three of the stars which Piazzi has classed among the Pleiades, viz. Nos. 170, 175, 179 are so far from the others, that they can hardly be said to form part of the cluster: unless, indeed, we extend the limits of this *subordinate constellation* (as it has been termed) to a space of 10 or 12 degrees in diameter! See a recent work, entitled " Wonders of the Heavens."

16 Tauri, or g Pleiadum.) Called *Celæno*. According to Herschel's estimate, the brightness does not exceed $6{\cdot}7^{m}$.

17 Tauri, or b Pleiadum.) Called *Electra*.

19 Tauri, or e Pleiadum.) Called *Taigeta*.

20 Tauri, or c Pleiadum.) Called *Maia*.

21 Tauri, or k Pleiadum.) Called *Asterope*.

P. III. 139, or *B*. 64 Tauri.) Piazzi calls this 15 n. Pleiadum, upon which supposition the R.A. of the Brit. Cat. requires +16′. Upon a recent examination it appeared to be 7th mag.

23 Tauri, or d Pleiadum.) Called *Merope*.

24 Tauri, or p Pleiadum.) There are two telescopic stars preceding this, the one $3^{s}{\cdot}5$, the other $1^{s}{\cdot}5$; both to the north.

25 η Tauri.) Called *Alcyone*, brightest of the Pleiades. Double according to Piazzi.

B. 104 Tauri.) Obs. by Bradley. Upon a recent examination appeared of the 6th magnitude. See also *Hist. Cél.* p. 36.

P. III. 153, or *B*. 105 Tauri.) This is called by Lalande 127 Mayer, in *Hist. Céleste*, p. 195; whereas in Mayer's Cat. the R.A. is +12′ 53.″ Decl. +2′ 22″. And upon a recent inspection, the star appears to be in the spot indicated by Mayer.

27 Tauri, or f Pleiadum.) Called *Atlas*.

28 Tauri, or h Pleiadum.) Called *Pleione*. According to Herschel's estimate, the brightness does not exceed 6 or 6·7 mag.

30 Tauri.) In Bode's Catalogue the Declin. is 9° too great. Double. Hers. III. 66. " Extremely unequal. L. w; S. r. Distance 11″.27; inaccurate. Pos. 17″·25 n. following."

P. III. 213, or *B*. 161 Tauri.) Proper motion −0″·19 in R.A. Double. Piazzi: the other star of 9th mag.; s. following.

P. III. 217, or *B*. 161 Tauri.) Proper motion −0″·26 in R.A. Nearly in the place assigned to 34 Tauri in the Brit. Cat., although it is well ascertained that the planet Herschel was the object actually seen by Flamsteed.

41 Tauri.) Is in Pigott's list of stars suspected to be variable, although he shows pretty clearly that there is no ground for the supposition. Piazzi expresses himself merely in two words "*fortasse variabilis*," without adducing any observations of his own on the point.

P. III. 261, or *B*. 190 Tauri.) Bode's Declination is nearly 42′ less than Piazzi.

Notes to the second Portion.

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50 ω. 2 Tauri.) Called by Piazzi a. 2 erroneously. A star of 8th mag. s. following.

56 Tauri.) Flamsteed's R. A. is right in the edition of 1712, but in that of 1725 requires −16′.

52 φ Tauri.) Double. Hers. V. 13. "Distance 55″·625, inaccurate."

54 γ Tauri.) Proper motion in R. A. +0″·14. It is called the first of the Hyades.

57 Tauri.) Proper motion in R. A. +0″·17, in Decl. +0″·10.

M. 142.) Proper motion in R. A. +0″·18, in Decl. +0″·02. See however Prof. Bessel's note.

59 χ Tauri.) Double. Hers. IV. 10. "Distance, 18″·75, very inaccurate."

60 Tauri.) Flamsteed's R. A. requires +6½′. Mayer's star 144 is correct.

62 Tauri.) Double. Hers. IV. 109. "Considerably unequal. L.w.; S.r. Distance 28″·1. Position 21°·2 north preceding." According to Piazzi, the smaller star is R.A. -1^s·8. Decl. +10″. Mag. 8.

B. 234 Tauri.) Is C. H. 370, and Bode supposes that this star with an error of -50^m in time, occasioned the insertion of 8 Tauri in the Brit. Cat. Burckhardt (*Conn. de T.* 1821, p. 307) is of opinion that N° 8 of Bessel's list of doubtful observations of Bradley, is the same star with this. He also accounts for the insertion of 8 Tauri, by attributing it to a miscalculated obs. of 104 Tauri.

68 δ. 3 Tauri.) Treble. Hers. VI. 101. "Has two stars in view. The nearest excessively unequal. L.w.; S.d. Dist. with 278, 63″·3. Pos. 35°·4 s. preceding. The furthest extremely unequal. S.r. About 1½ minute. Pos. about 50° n. preceding."

Anon. R.A. 63° 50′) *Hist. Céleste,* 195. Double. Hers. II. 54. Near ½° s. preceding ε Tauri, in a line parallel to α and γ; a small star. Extremely unequal. L.r.w.; S.d. With 460 above 3 diameters of L. Pos. 68°·7 s. preceding."

Anon. R.A. 63° 53′.) H. C. 195. Double. Hers. IV. 74. "Near ½° n. following 68 δ. 3 Tauri, towards ι. Very unequal. L. pr. S. r. Distance 16″·5. Pos. 25°·75 n. following."

74 ε Tauri.) Called *Ain.*

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77 and 78 ϑ Tauri.) According to Bradley's observations compared with Piazzi the proper motion of ϑ. 1 Tau. is R.A. −0″·04;

—0″·04; Decl. +0″·06. That of ϑ.2, R.A. +0″·17, and Decl. +0″·10. But Bessel upon examination of Fl. obs. finds the distance of the two stars in his time to be as nearly as possible the same as determined by Piazzi; whence arises considerable doubt as to the correctness of the proper motions above stated.

80 Tauri.) Mayer's declination requires —5′.

M. 160.) In the first edition of Piazzi's Catalogue, the declination is 1° too little, and it is called 82 Tauri, which latter star has no existence. Bode 264; 265, 266 are one and the same.

87 *α* Tauri.) Double. Hers. VI. 66. "Extremely unequal. L.r.; S.d. Distance 87″·75. Pos. 52°·97 n. following." Sir William measured the apparent diameter of the large star; with 460, it was 1″·8; with 932, 1″·2; disc well defined. With respect to the proper motion of the star, Bessel makes it in R.A. +0″·04; in Declination —0″·10. Bouvard (*Conn. de T.* 182[illegible], p. 292) makes it +0″·147 in R.A., which increase is probably occasioned by assuming the precession less than that of Bessel. Bouvard's position is given in the present catalogue. That of Piazzi is —0″·9 in R.A., in declination exactly equal.

Anon. R.A. 66° 32′) Double. Lalande, *Hist. Céleste*, page 204.

94 *τ* Tauri.) Double. The smaller star, 158 of Piazzi, 8th mag. R.A.—31″·5; Decl.—50″·9. Hers. VI. 7. "Distance 71″·42, pretty accurate."

95 Tauri.) Fl. R.A. requires — 12′. It is 140 of Lacaille's Zod. Cat.

9 *o*. 2 Orionis) In Piazzi the character is misprinted o.

M. 180.) Piazzi supposes this to be the star intended, as 100 of the Brit. Cat. which is there laid down R.A. +9′, Decl. —47½′. On the other hand, Herschel has pointed out an observation of 100 Tauri in Fl. *Historia Cœlestis*, and to all appearance it is a good one, though the star is not now to be found in that place; and he therefore concludes it to be lost.

M. 181.) Treble. One of the small stars, 255 of Piazzi, R.A. —39″·2. Decl. +22″·2. Mag. 8. Hers. V. 57. "More than one degree n. following 9 Orionis towards 113 Tauri; the largest of two. The two nearest considerably unequal. L. rw. S. rw. Distance with 278, 30″·43. Pos. 33°·6." And again V. 113. "About 1½° s. prec. 11 Orionis, towards *ι* Tauri §. L.w.; S. pr. Distance 37″·85. Pos. 33°·9 n. preceding. The third further off and smaller, S.r. Pos. n.

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§ Herschel's descriptions of the place of this treble star do not accord with its real place. It seems we ought to read them thus: More than 1° n. foll. 9 Orionis, towards *ι* Tauri; and, about 1½° s. prec. 11 Orionis, in a line continued from 113 Tauri.

following." The mean of Herschel's measures give for the diff. of R.A. of the two nearest, − 31″·86. Decl. + 20″·63.

101 Tauri.) Fl. R.A. requires − 8′. Mayer's 182 is right.

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104 m′ Tauri.) Proper motion by good observations, of Bradley and Piazzi, In R. A. + 0″·72, in Decl. + 0″·07; in a great circle 0″·685.

C. 152.) Bode and Piazzi suppose this to be the same with 103 Tauri of Flamsteed, whose R. A. in that case requires + 12′. Herschel calls his double star V. 114, by the name of 103 Tauri; and in a note to his Catalogue of comparative brightness, he remarks that although Fl. has no observation of it, yet his (Sir W's) double star cannot be far from the place pointed out by the Br. Cat. Herschel describes the double star above mentioned, thus "Excessively unequal. L. r. w.; S. d. Distance with 278 and 625, 30″·03, mean measure. Position 72°·4."

105 Tauri.) Double. Hers. VI. 105. Very unequal. L. p. r.; S. r. Distance 101″·5. Pos. 18°·0 s. prec."

Anon. R. A. 74° 18′) This may probably be Herschel's double star III. 90. "About 3° directly north of 103 Tauri; the largest of three forming an obtuse angle. Considerably unequal. L. r. w; S. p. r. Distance with 278, 13″·1. Pos. 64°·0 n. following." As to the identity of the star to which Herschel makes reference, see the last note but one. See also Lalande. *Hist. Céleste* p. 139 as to the position of the supposed double star; which, if the above description be rightly understood, is the middle star of the three, of which *P.* IV. 298 is the northern and preceding.

C. 156.) Is the same with 160 of Caroline Herschel's Catalogue.

14 (a) Aurigæ.) Double. Hers. IV. 19. "Very unequal, L. r. w.; S. d. Distance 16″·13, a little inaccurate. Pos. 37°·63 s. preceding."

Anon. R. A. 76° 0′) *Hist. Cél.* p. 134. Supposed to be Herschel's star II. 48. "A minute double star. Less than ¼ degree s. prec. 16 Aurigæ, in a line parallel to 10 and 8; the preceding star of a small triangle of which 16 is the largest and following. A little unequal. Both p. r. With 227, 1¼ or when best, 1½ diameter of L. Position 15°·8 n. following."

M. 197.) Mayer's R. A. requires − 16′.

P. V. 43.) Piazzi thinks this may be 195 of Mayer, which star is left out of Wollaston's Catalogue.

P. V. 55 to 14[illegible] inclusive.) A singular error pervades the ninth column of Piazzi's Catalogue, within these limits. The numbers are all placed one line too high. The precesson in Declination for the first of the above stars should be 4·28.

111 Tauri.) Proper motion in R. A. +0″·17. Double. Hers. V. 110. "Very unequal. L. r. w. S. r. Distance 46″·7. Position 3°·8 n. preceding."

112 *β* Tauri.) Called *Nath*. Proper motion in R. A. + 0″·10, in Decl. —0″·20.

113 Tauri.) Position according to Bradley. Piazzi has it not, neither does he mention having ever looked for it. Both Lalande and Herschel observed it.

115 Tauri.) An approximate declination, erroneous to the extent of 10′, is given in Bode's Catalogue, and marked L.; yet the declinations of Fl. and Meyer are agreeable to truth.

114 ο Tauri.) Double. Hers. V. 115. "Excessively unequal. L. w. S. a point. Distance 5″·57. Pos. 77°·9 s. preceding. Two other small stars following, and a third to the north." In the place quoted, for *o* read o.

117 Tauri.) Double. Hers. III. 93. "Almost equal. Both r. w. Distance 12″·2. Position 52°·45 s. following."

Anon. R. A. 79″ 9′) *Hist. Cél.* p. 262. R. A. from 2 obs. of Bradley.

118 Tauri.) Double. Hers. II. 75. "A little unequal. L. w.; S. w. inclining to r. With 278, 2½ diameter of L, by the micrometer 4″·7; more exactly with 625, 5″·03. Pos. 77°·25." Sir William "could just see it with an 18-inch achromatic, made by Nairne, it was as close as possible, and a pretty object."

Anon. R. A. 79° 20′) *Hist. Cél.* p. 260. Double. Hers. IV. 110. "About 1¼ deg. n. following *β* Tauri, towards ϑ Aurigæ; the second in that direction. Very unequal. L. r. S. d. Dist. 16″·02. Pos. 74°·9 n. preceding." The star preceding it is doubtless *P*. V. 99.

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120 Tauri.) A star of a ruddy colour precedes this.—*Piazzi.*

35 Orionis.) The R. A. is marked :: in Fl. and requires—3′. Mayer's R. A. requires + 26″ Decl. — 1′ 20″.

Anon. R. A. 81° 6′) *Hist. Cél.* p. 36. Supposed to be Herschel's star I. 70. "A very pretty double star. Near 1° n. preceding ζ Tauri towards Capella, the corner of a rhomboid made up of ζ, this, and 2 more, and opposite to ζ. Considerably unequal. L. p. r.: S. a little deeper r. With 227, almost 1 diam. of L.; with 460, 1½ diam. Pos. 36°·4 s. preceding."

26 Aurigæ.) Double. Hers. III. 64. "Very unequal. L. r. w.; S. r. Distance 13″·4. Pos. 2°·6 n. preceding."

P. V. 164.) Piazzi calls this 124 Tauri, while Herschel is of opinion that no such star was obs. by Fl. unless with a correction of + 1° 4′ of R. A. which would make 124 Tauri identical with *P*. V. 192.

126 Tauri.) Is misprinted 116, in Piazzi's Cat.

P. V. 184.) Three stars near this, the first 8 mag. n. prec. the second 7.8 mag. s. prec., and the third 8.9 mag. following.

P. V. 192, or *B*. 394 Tauri.) Is *C*. *H*. 355. See the last note but two.

128 Tauri.) Proper motion, R. A. − 0″·14. Decl. + 0″·10.

M. 216.) Mayer's declination is − 26″, it was observed by him but once.

133 Tauri.) The Decl. in Bode's Cat. is only to minutes, is marked L, and is 5′ too great; yet Flamsteed's declination is within a minute of the truth.

M. 218.) Bode's 191 Orionis does not agree very well with this. R. A. − $4\frac{1}{3}'$, Decl. + $4\frac{1}{4}'$, and marked L. Mayer's R. A. is marked ∴.

P. V. 225.) Hereabouts, a double star. Hers. I. 67. "About 55′ preceding the 37th nebula of M. Messier; the largest and most preceding of 2 stars. Very unequal. Both p. r. With 460, near 2 diameters of L. Pos. 23°·95 n. following." The place of the nebula or more properly cluster, above referred to, is about R. A. 84° 50′, Decl. 32° 13′.

P. V. 242.) The R. A. of this as obs. by Bradley, is − 8″·9 when compared with Piazzi's determination; that of the next star (244) is + 8″·9. Upon which Bessel remarks, that although Br. observed each star only once, yet he thinks some reliance may be placed on their proper motions. Bradley's diff. of R. A. for 1755, = 79″·6: Lalande for 1798, (*Hist. Cél.* 313) = 4^{s}·5 = 67″·5; Piazzi, for 1800, = 55″·2.

P. V. 244.) Pi. supposes this to be 138 Tauri. In Fl. obs. of that star, the time is wanting, but Herschel thinks the R.A. of the Brit. Cat. not far from the truth.

137 Tauri.) Appears to be double.—*Piazzi.*

54 χ. 1 Orionis.) Proper motion in R.A. − 0″·23, in Decl. − 0″·09.

57 χ. 2 Orionis.) Proper motion in R. A. + 0″·12, in Decl. + 0″·08. In Piazzi's Catalogue it is erroneously called 64 χ. 4.

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Anon. R. A. 87° 15′) *Hist. Cél.* p. 315. Quintuple. Hers. IV. 48. "In the form of a cross. About ¼ degree n. preceding h Geminorum, in a line parallel to 65" (qu. 62?) "Orionis and ζ Tauri; the middle of three. The two nearest or preceding of the five extremely unequal. Distance 20″·95. Pos. 7°:45 s. preceding. The last of the three, in the short bar of the cross, has an excessively obscure star near it of the third class. Five more in view, differently dispersed about the quintuple.

P. V. 300) This star was obs. by Piazzi when looking for 231 of Mayer, which he could not find.

64 χ. 4 Orionis.) Fl. R. A. requires $-45'$. Bode calls this χ. 3.

62 χ. 3 Orionis.) Bode calls this χ. 4.

P. V. 328.) The place of Bode's 256 Orionis does not agree very well with this, the R. A. being $+2' 51''$: Decl. $+41''$.

M. 234.) Mayer's position is derived from an imperfect observation.

4 Geminorum.) Mayer's R. A requires $-17''\cdot4$. Decl. $-12''\cdot3$; it was obs. by him but once.

Anon. R. A. 89° 41′) *Hist. Cél.* p. 262. Double. Hers. VI. 114. "About ½ degree s. preceding 69 Orionis, nearly towards λ. Considerably unequal L. p. r; S d. Distance 90″·63. Pos. 22°·1 s. following."

⁂ With reference to what is stated at page 127 of the present volume, it may be proper to mention, that the Zodiacal Stars of Wollaston's Catalogue, which are omitted in this, consist of such as are not now to be seen in the heavens, and which there is good reason to suppose never to have existed, but to have been inserted in the original catalogues through miscalculation. The list of such stars is deferred until the completion of the present Catalogue.

P. S. The compiler takes the liberty of mentioning, that should any person be in possession of unpublished materials which may serve to enrich this Zodiacal Catalogue, although not of sufficient importance for separate publication; he shall be happy to avail himself of them, on their being communicated through the Editor.

LXXVIII. *On the Decomposition of Metallic Salts by the Magnet.* *By Mr.* J. Murray.

In my Paper "on the decomposition of metallic salts by the magnet" transmitted to the Royal Society of Edinburgh I referred to experiments which seemed to me unequivocally to prove the influence of magnetism in the decomposition of metallic salts —I continue to receive renewed evidence of the truth of my conclusions—I shall here take leave to select a few of the numerous experiments repeated in the course of my researches, and it would, methinks, be difficult to summon any objection to them; I confess that they appear to *me* quite satisfactory.

A solution of permuriate of mercury was by the magnet soon reduced into running or metallic mercury, and the supernatant fluid was not affected by the albumen of the egg.

Hence, fine steel filings magnetized and administered in sirup will be an admirable antidote to corrosive sublimate.

Nitromuriate of platinum was decomposed with a brisk effervescence distinctly audible and with a visible spray between the eye and light.

Fine Dutch steel wire was selected, and proved to be non-magnetic.—It was thrown into nitrate of silver where it remained for 14 hours without being affected, part of this was made the uniting wire between the N. and S. poles of 2 bar magnets; when, it became speedily plumed with crystals of silver.

A portion of the same wire was snapped in twain and the magnet passed over one of the fragments and both projected into solution of nitrate of silver—that which was magnetised reduced the silver, while the other remained inert.

The magnetic bar was coated with copal varnish and placed into solution of muriate of mercury, but reduction took place as if no such film had interposed.

Two magnetic bars were left for 2 days in phosporous acid. The acid was decomposed—the north pole of one of the bars was scarcely affected, but the south pole of the other was corroded $\frac{1}{8}$ inch deep, and developed the fasciculated structure described by Mr. Daniel.

The two magnetic poles (N. and S.) of two bar magnets immersed in nitrate of silver were united about $\frac{1}{2}$ inch from their extremities by a thread of steel; a precipitation of crystals of reduced silver took place about the uniting wire (very few below) and the uniting wire itself became so invested.

I have succeeded in decomposing every metallic salt in this way to which I have applied the magnet; and I have yet to be informed that steel, simply as a carburet of iron, will attract all acids whatever from every metal whatsoever.

A portion of platinum wire that suffered no change in nitrate of silver, in solution, was made the uniting wire between the poles of a powerful horse-shoe magnet (that supported 12lbs. weight). When this was immersed into nitrate of silver it soon became discoloured and acted upon.

When a magnetic bar is plunged into solution of nitrate of silver it accomplishes its complete reduction, however considerable the quantity, the surface of the magnet in contact with the solution is not abraded, but the surface above the solution is much corroded from the escape of the acid vapour, the consequence of decomposition.

When in the nitrate of silver the N. pole became instantly studded with brilliant pallets of silver, and formed more rapidly and more copiously round it than round the south pole. These crystalline pallets exhibited evident polarity, and were affected by the approach of a fine steel plate.

When the magnet is plunged into a solution of muriate of mercury, and the decomposition takes place which yields globules of fluid metallic mercury, it will be seen that the action is most intense at the angles and base of the bar, and the reduction there more copious and prompt. This phenomenon is manifested when a magnetic bar is rolled in iron filings; for it will then be perceived that the quantity of adhering particles is much greater in these places than in other parts of the surface.

It is an interesting spectacle to witness the reduction of minute metallic balls around the poles, particularly the north and its base, with a square floor reflecting the form or impress of the inclined bar—the reduction commences at the edges, and is striking and beautiful.

J. MURRAY.

LXXIX. *Notices respecting New Books.*

The Imperial Almanack; or, Annual Compendium of Astronomical, Statistical, Scientific, and Interesting Information, for the Year of our Lord 1822.

We conceive it to be quite within the scope of our duty as journalists of science, to notice an Almanack which comes forth with any pretensions to the character of scientific. Most of the Almanacks published in this country are sad indications of the ignorance which still prevails among our peasantry, ignorance which can find gratification in the perusal of astrological predictions, and which can tremble or rejoice in the expectation of events pretended to depend upon the mutual aspects of the moon and planets. Among the Almanacks published by the Stationers' Company, there are a few exceptions to this censure; especially those widely-circulated productions the *Ladies' Diary*, the *Gentleman's Diary*, and *White's Ephemeris*; the latter of which, we are glad to observe, has lately received some valuable improvements.

The *Imperial Almanack*, to whose first number we now beg leave to draw the attention of our readers, presents several new and interesting features. The *calendar* part, which occupies 24 pages, two to each month, exhibits all the usual matter of an Almanack, such as the lunations, the anniversaries, holidays, &c. the times of rising and setting of the sun and moon, and of the moon's southing; contains also, a column for the sun's right ascension and declination; and comprises, instead of the usual column entitled "equation of time," one that shows *the mean* *time of apparent noon*; or in other words, the time that will be indicated by a clock truly regulated when the sun is on the meridian. This, though a very simple change, is a real improvement.

At the foot of the calendar pages, we have, for every sixth day, the declination, time of southing, and *meridian altitude* at London, of the principal planets. The last is, evidently, a very useful column. The declination and time of culmination of Ceres, are given for every *eighth* day. The principal astronomical facts and phenomena that will occur in each month are duly announced: and there is further for each month "*a ruled page to facilitate the keeping of a meteorological register.*

The remainder of the Almanack is miscellaneous, containing a variety of interesting synoptical tables, so arranged as to comprise much in comparatively small compass. After a brief notice of the eclipses and of the approaching transit of Mercury, there are inserted short accounts of the Jewish, Mahometan, and Roman calendars; the two first of which are suited to the year 1822. Then follow in the order here specified, the Elements of the Solar System; a comprehensive table of Terrestrial latitudes and longitudes; a general survey of the earth; tables of the population and cultivation of Great Britain, of the principal cities and towns, of the colonies and dependencies; the number of British peers at different periods; amount of revenue at different epochs, of national debt at ditto; a syllabus of employments, and a view of the value of exports and imports. Next to these are given tables of bishops, deans, &c. with the extent, and numbers of prebendaries, canons, livings, &c. in each diocese, and of the principal dimensions of the English cathedrals. Other tables relate to the probabilities of life, the London Bills of Mortality, the altitudes of mountains in different parts of the world,—of perpetual snow in different latitudes, of edifices. Then we have three interesting chronological tables, of which one exhibits the dates of geographical discoveries, one the dates of astronomical discoveries, the other of astronomical and nautical inventions. Among these we were glad to observe in their proper places, the dates of the invention of Davis's sea-quadrant, of Norwood's measurement of a degree, of Hutton's computation of the earth's mean density, of Barlow's magnetical discoveries, and of the invention of Dr. Pearson's micrometer. Parry's arctic discoveries, and Smith's discovery of South Shetland, are also very properly recorded. The five last tables relate to specific gravities. Thermometric criteria of interesting chemical phænomena, European itinerary measures, value of English coins at different epochs, and curious results of computations and experiments.

The Editor of this Almanack states, in a concise preface, that " he has been desirous to draw into a narrow compass much useful information on several topics of general interest amongst well-informed men of all classes. He has aimed at correctness as well as utility, and hopes that, to a considerable extent, both objects have been attained. In our judgement his attempt is completely successful; and we the more cordially recommend his production to our readers in general.

We have heard it rumoured that this Almanack has been composed by Dr. Gregory, Professor of Mathematics in the Royal Military Academy: in this case we know not why he should withhold his name, as so useful a compendium is not likely to deduct any thing from that gentleman's well-earned reputation.

Lately published.

A Treatise on Smut in Wheat. By Francis Blakie. 1*s*. 6*d*.

A View of the Agriculture, Manufactures, Statistics, and State of Society of Germany, and Parts of Holland and France; taken during a Journey through those Countries in 1819. By William Jacob, Esq. F.R.S. 4to. 1*l*. 5*s*.

Essentials of Modern and Ancient Geography. 18mo. 4*s*.

Observations on the Idiom of the Hebrew Language. 8vo. 6*s*. 6*d*.

Notes relating to the Manners and Customs of the Crim Tartars; written during a four years residence among that people. By Mary Holderness. 12mo. 5*s*. 6*d*.

A Tour through North Wales; illustrated with 40 select Views, engraved and coloured from the originals of Messrs. Turner, R.A. 5*l*. 5*s*.

Craig's Lectures on Drawing, Painting, and Engraving, delivered before the Royal Institution. 8vo. With Plates and Woodcuts. 14*s*.

A Practical Treatise on Gutta Serena. By John Stevenson. 8vo. 7*s*. 6*d*.

The Natural History of British Quadrupeds. By E. Donovan, F.L.S., &c. with coloured Plates. 3 vols. royal 8vo. 5*l*. 8*s*.

No. II. of Illustrations of British Ornithology. By P. J. Selby, Esq. Folio. 1*l*. 11*s*. 6*d*., or finely coloured 5*l*. 5*s*.

Illustrations of the Linnæan Genera of Insects. By W. Wood, F.R.S. With 86 coloured Plates. 2 vols. royal 18mo. 1*l*. 10*s*.

Travels in Palestine, through the Countries of Bashan and Gilead, East of the River Jordan; including a Visit to the Cities of Geraza and Gamala, in the Decapolis. By J. S. Buckingham, Esq. With Maps and Plates, 4to. 3*l*. 13*s*.

Count Romanzoff's Voyage of Discovery into the South Sea and Behring's Straits in 1815, 1816, 1817, and 1818. 3 Vols. 8vo. 2*l*. 5*s*.

A Voyage to Africa, including a particular Narrative of an Embassy to one of the Interior Kingdoms in 1820. By Wm. Hutton, late acting Consul for Ashantee, &c. With maps and plates. 8vo.

Preparing for Publication.

Travels in the Interior of Southern Africa. By W. Burchell, Esq.

Mr. Peter Nicholson's System of pure and mixed Mathematics, in one large volume, for Schools, will appear in two or three weeks.

Typographia; an Historical Sketch of the Origin and Progress of the Art of Printing: with Details of the latest Improvements, Stereotype, Lithography, &c. By T. C. Hansard.

Dr. Leach will speedily publish the Synopsis of British Mollusca, illustrated with plates.

Mr. Freind's annual volume of Evening Amusements on Astronomy will appear at the end of the year.

A New Practical Treatise on the *Sliding Rule*, in Two Parts, is in the press, and nearly ready for publication. The First, as a general Introduction to the use of common Sliding Rules: Second, a Collection of useful Formulæ for the scientific calculator.

The New "Society of Practical Medicine of London" intend, we understand, to publish their Transactions quarterly; and the first Number will be published in January.

LXXX. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY OF LONDON.

Nov. 9. THE Meetings of this Society commenced this evening. A letter was read, from Dr. Pearson, announcing some observations of the occultations of the *Pleiades* by the Moon on July 23 and October 13. A communication was also made from M. Piazzi, relative to the late solar eclipse, and detailing the result of his observations. A paper from Mr. Herschel was read, giving an account of the mode of dividing astronomical instruments as practised by M. Schenck of Berne in Switzerland, one of the pupils of the celebrated Reichenbach. The present state of peace has afforded opportunities of witnessing several of the productions of these distinguished artists: and they are found (if not to excel, at least) to rival the best works of the English artists. They are finished with a delicacy of execution and touch unknown to most people in this country. Some expectation is held out that M. Schenck may be induced to visit this metropolis: and to make it the theatre of his future labours. The contents of this paper were highly interesting to the practical mechanic and to the scientific astronomer: but it cannot well be abridged in a journal of this kind.

LXXXI. *Intelligence and Miscellaneous Articles.*

COMMUNICATIONS FROM MR. JOHN MURRAY.

“ *Rabies canina.*—At page 311 of your October Number, you state, on the authority of a correspondent in the Medical and Physical Journal, that the *rabies canina* invariably affects the *male* dog, and never the female. This assertion, however, allow me to state, is *false.* I have, myself, been recently connected with experiments on *two mad dogs.* The first was a *pointer bitch*, and the other a pointer dog bitten by the former. The dog exhibited all the phænomena of the sullen madness, and the bitch those of the “ biting madness,” so very accurately described in both cases by the ingenious author of the article “Dog” in Rees's Cyclopædia.

Calculous Diseases, &c.—I had frequently noticed the interesting fact that Mr. Dalton has adverted to, in the action of waters containing supercarbonate of lime on vegetable blue colours; but, “ devotion to established authority” induced me to attribute the phænomenon to the presence of an *alkaline carbonate.* I observed this first in my analysis of the mineral spring adjacent to the Temple of Serapis near Pozzouli.

In analysing lately some rain-water from a rain gauge fixed apart from buildings, I detected a minute portion of *lime*: and as I find that tincture of cabbage exposed to the atmosphere soon exhibits a film of a *green* colour, I am disposed to attribute the change to the presence of *supercarbonate of lime.*

I may be permitted to add, that I have invariably found *calculous diseases* most prevalent in districts where the water contains *sulphate of lime*; and an almost total absence of the disease where the springs exhibit *supercarbonate of lime* on analysis. The *County of Norfolk* is an example of the former, and *Holderness* an instance of the latter.

The Diamond.—The following phænomena may be deemed interesting in reference to the physiological history of the diamond:

By repeatedly exposing a diamond to the action of the oxy-hydrogen blow-pipe in a nidus of *magnesia*, it became as *black* as charcoal, and split into fragments which displayed the *conchoidal* fracture.

It will be found, that this gem affixed in magnesia soon flies off in minute fragments, exhibiting the impress of the conchoidal form.

In lately exposing the diamond fixed on a support of pipe-clay to the ignited gas, I succeeded in completely *indenting* it:—

examined after the experiment, it exhibited proofs of having undergone *fusion.*

Phosphorus in Ether.—I had thrown a number of chips of phosphorus into ether, in order to form phosphorized ether. After a considerable lapse of time, I found these chips curiously incrusted with transparent acicular crystals bearing a remarkable resemblance to the incipient germination of the barley-corn in the process of malting. Incidental agitation unfortunately destroyed them.

Magnetism.—A small bar magnet being allowed to remain immersed in tincture of cabbage for two or three days, completely *destroyed* the blue colour, and the same thing occurred with that of litmus.

The two legs of a horse-shoe magnet (about 3-4ths inch apart) were placed *separately* in small cylinders, each containing solution of nitrate of silver—around one of the poles thus separated a dark cloud collected, and a few solitary crystals studded the other on the side nearest to that of its adjunct.—Little alteration was exhibited after a lapse of two days. But both poles being placed together in a vessel with the same metallic solution, soon effected a complete decomposition, which was exhibited by both the poles becoming completely clothed in brilliant metallic silver, while sparkling minute crystals of the same floated through the liquid, which, from being previously colourless, had become *coloured.*

Steam Drying Rooms.—Dr. Ure has stated in his "Nicholson's Dictionary of Chemistry," that "the people who work in steam drying rooms are healthy; those who were formerly employed in stove-heated apartments became soon sickly and emaciated. These injurious effects must be ascribed to the action of cast iron at a high temperature on the atmosphere.

I remarked that among the Appennines the Italians place a *shallow earthen vessel supplied with water* on the head of the stove, the pipe of which traverses the apartment, and, on inquiring the reason, have been repeatedly assured, that without it they should be subject to head-ache and other ills—while with this simple precaution they experience no inconvenience whatever.

I have deemed it right to mention this, as it points out a very simple yet effective remedy.

Nov. 15, 1821."

POLAR EXPEDITION.

Letters have been received from the 'Discovery Ships,' dated 16th July; they were then at Resolution Island in Hudson's

Bay; they had met with some heavy icebergs, and considerable obstructions from the ice, which was then melting fast, but were past these inconveniences, and pursuing their voyage of discovery up the inlet at the north of the bay. The officers and men were all in the highest health and spirits, and most amply found in every kind of provision and comfort, and delighted with the security and excellence of their ships; which, though so deeply laden, had proved themselves most lively and obedient sea boats.

ARCTIC LAND EXPEDITION.

Soon after the expedition under Lieutenant Franklin, R. N. had arrived on the coast of Hudson's Bay, they proceeded from York Factory, the grand depôt of the Hudson's Bay Company, towards their wintering ground at Cumberland, the central post of the interior, a distance of about 900 miles from the coast.—Lieutenant Franklin, Dr. Richardson, Mr. Back, and Mr. Hood, attended by the hardy Orkneymen who had been engaged to man the boats in the rivers of the interior, had worked in the Company's service several years, and understood the language of many of the Indian tribes, left the factory on the 7th of September 1819, with a fair wind, under a salute from the depôt, and amidst the acclamations of the officers and men of the Company. Of the immense quantity and variety of provisions supplied by Government for the use of the expedition, the greater part was left at the factory; those who knew the country, and the difficulty of travelling through it, having represented the impossibility of conveying European food, which at the Bay receives the name of luxuries, to any considerable distance. The hardships attending the progress of travellers were, in fact, shown to be so great, as would render it absurd to calculate upon such a thing as the slightest change of diet in the winter season; and when it was mentioned by Lieutenant Franklin, that he had brought with him preserved meats and soups in portable cases, to support the expedition in the cheerless regions through which they were to pass, there was a general laugh amongst the officers of the Company, at the idea of associating any thing like comfort with the formidable character of the enterprise. Some of these difficulties may be estimated from the account of the sufferings of the adventurers, in their advance towards Cumberland, to which place the writer of this article accompanied them. On the third day after their departure from the factory, the boats of the Company, which were proceeding to the various trading-posts in the interior, came up with the expedition in the Steel River, distant about sixty miles from the place at which they set out. Most of the rivers in that part of America abound with rapids and falls. The rapids are generally more navigable near the banks, but they frequently extend across the stream, and then the labour of the boat's crew becomes excessive, every man being obliged to turn into the water and assist in carrying the boat sometimes to the distance of half a mile before they gain the head of one of those terrible impediments. The Company's men, upon turning one of the points of the river, observed the officers of the expedition making desperate efforts to get through the mud along the banks; some of them were up to their knees, others up to their waists, while the men were handing the boats over a most violent rapid, which, though but half a foot deep, rendered it necessary that those who stood in the water should hold fast by the boat, the impetuosity of the stream being so extraordinary as not unfrequently to overturn a man in an instant, and dash him to pieces against the rocks and huge stones which lie scattered along the bed of the river. Indeed, before the Company's boats had reached those of Lieutenant Franklin, it was suspected that the expedition had already met with more hardships than they had any notion of encountering at so early a period. Several of the tin cases which had contained the preserved meats were seen at the different *up-putting* places (the spots of ground on the banks chosen for passing the nights upon), and those miserable abodes were drenched with rain, and presented an appearance the most appalling. Two black bears were seen prowling about, and devouring some of the luxuries which the travellers had ascertained it was impossible to convey, in any considerable quantities, further up the river; and along the banks were seen strong symptoms of the inexperience of those who had gone forward. The traders with the North American Indians, in travelling to their posts, kindle fires of immense magnitude upon landing to put up for the night. Every man carries his fire-bag, containing all the necessary apparatus. They proceed to hew down the trees, an office which they perform with wonderful dexterity. The fires are lighted, the tents for the officers pitched, and the only regular meal taken during the 24 hours, served up in as comfortable a manner as possible under the circumstances.

As the travellers advanced, the mild season not having yet begun to disappear, vast herds of grey deer were observed passing the rivers towards the Esquimaux lands; and the Indians who were accompanying the expedition gave extraordinary proofs of their activity, by rushing upon the animals in the water, and striking long knives into their hearts.—Lieutenant Franklin, on entering the Hill River, so called from a neighbouring eminence, the only one that presented itself between York Factory and Cumberland, had reason to express surprise that trading goods could be transported to the interior in spite of such frightful obstructions. His men were fatigued in the extreme, and he found it indispensably necessary to request that the officers of the Hudson's Bay Company would lighten his boat of the greater part of the luxuries and instruments. This accommodation was readily given; and after the most laborious efforts, the expedition reached the Rock depôt, one of the Company's posts, having devoted seven days to the exhausting toil of working up thirty miles of their journey. Upon arriving at the depôt, the expedition were treated with great hospitality by Mr. Bunn, the officer in charge, who entertained them with the tittimeg, a fish which they admitted was the most delicious they had ever tasted, and which was caught in God's Lake (an immense piece of water, so named from the abundance and excellence of its inhabitants).

Mr. Hood, who is one of the draftsmen of the expedition, took a sketch of the Rock Fall and the Post, which presented one of the most beautiful objects in these desolate regions, and introduced a distant view of a wigwam (an Indian tent) with its inmates. Five days after the expedition left the Rock depôt, they reached another post, having encountered numberless difficulties similar to those which have been described. There was, however, some relief to the painful sameness of the journey, in several beautiful lakes through which they had to pass. At Oxford House post, which was reached four days subsequently, they were provided with *pimmikin*, the celebrated winter food of the country, made of dried deer or buffalo flesh pounded and mixed with a large quantity of the fat of the animal. This food is substituted for the luxuries, in winter, is the most portable of all victuals, and satisfies the most craving hunger in a very short time. The officers of the expedition were not a little surprised at the difficulty of cutting their meat, but they soon reconciled themselves to the long-established practice of chopping it with a hatchet. During the summer, ducks, geese, partridges, &c. are to be had in the greatest abundance; but the frost soon drives all these delicacies out of the reach of the active Indian, and *pimmikin* becomes the only resource of the traveller. The next post at which they arrived was Norway House; upon leaving which they entered upon Lake Winnipic, at the further side of which they had to encounter the grand rapid, extending nearly three miles, and abounding in obstructions quite insurmountable. Here they were obliged to drag their boats on shore, and carry them over the land, or, to use the technical language, "launch them over the portage." The woods along the banks were all in a blaze, it being the custom of the natives, as well as of the traders, to set fire to the trees around the up-putting places, for the double purpose of keeping off the cold and the wolves, whose howling was increased in proportion to the extent of the conflagration.

The expedition passed several other rapids and falls along a flat, woody, and swampy country, across five miles of which no eye could see. At length they reached the White Fall, where an accident took place which had nearly deprived the expedition of their commander. While the men were employed in carrying the goods and boats across the portage of the fall, Lieutenant Franklin walked down alone to view the rapid, the roaring of which could be heard at the distance of several miles. He had the boldness to venture along the banks with English shoes upon his feet, a most dangerous experiment where the banks are flintstones and as smooth as glass. He was approaching the spot from which he could have taken the most accurate observation, when he slipped from the bank into the water. Fortunately the water into which he was precipitated was still water. Had he lost his footing ten yards lower down, he would have been hurried into a current which ran with amazing impetuosity over a precipice presenting one of the most terrific objects his eyes had yet fixed upon amidst all the horrors of the journey. Lieut. Franklin is an excellent swimmer, but he had on him a sailor's heavy Flushing jacket and trowsers, heavy English shoes, and a large neck-handkerchief, the weather having begun to set in very cold. He swam about for some time, and made vigorous efforts to get upon the bank; but he had to contend against a smooth precipitous rock, and was just exhausted when two of the Company's officers, who were at a short distance from the fall, looked up and saw him struggling in the water. With the assistance of their poles they raised him out of his perilous situation, in which he had been nearly a quarter of an hour. The moment he reached land he fell to the ground, and remained without motion for some time. His powerful constitution, however, soon buffeted the effects of the accident, and he had happily only to regret the injury his chronometer, for which he had given 100 guineas, received in the water. After a tedious journey of forty-six days, the dangers and distresses of which rather increased than diminished as they advanced, the expedition arrived at Cumberland, a post situate on the banks of a beautiful lake, and stockaded against the incursions of savages, the attacks of wolves and bears, and the more ferocious assaults of rival traders.

Further particulars of the progress of the Expedition are detailed in the subjoined letter written by one of the Officers attached to it:

North America, Lat. 64. 28. N. Long. 113. 4. W.

"The public papers have probably informed you of the arrival of the Northern Land Expedition in Hudson's Bay, in September 1819, after an escape from shipwreck. It proceeded from thence to Cumberland House, one of the Hudson's Bay Company's settlements, nearly half way across the continent, this being considered the best route in order to reach the sea at the mouth of the Copper Mine River. Here the winter of 1819 was passed. The depth of snow, and the severity of the cold, during the almost interminable winter in this country, precluded the possibility of conveying heavy stores, as only one-third of the year can be employed with any advantage by the traveller.

"The time, however, was not lost; we employed it in making drawings of animals, birds, &c. charts, meteorological observations, and collections of specimens, which we transmitted to England in the ensuing spring.

"In June 1820 we set forward in canoes manned by Canadians. The extreme heat of the short summer, the persecutions of noxious insects, and occasional want of food, are the usual [illegible] of these voyages; obstacles insignificant in compa[illegible] with the formidable difficulties which we have yet to overcome. On the 29th July we arrived at the north side of the Slave Lake. A party of Copper Indians were engaged to accompany us, and we commenced the work of discovery. On the 1st of Sept., we reached the banks of the Copper Mine River, in lat. 55. 15. [illegible] 113. W., a magnificent body of water two miles [illegible]

"We [illegible] into a country destitute of wood, and our men were [illegible] with the labours of carrying canoes, cargoes &c, amounting to three tons, from lake to lake. Their broken spirits were revived by our success; but the season was too far advanced to make any further progress. We returned to a small wood of pines, and erected our winter residence of mud and timber, which we have named Fort Enterprise.

By Indian report this river runs into the Northern Sea, in West. longitude 1[illegible]; and, we suppose, in lat. 72. In June 1821 we shall embark, and the river will enable us to reach the sea in a fortnight. If the shore is encumbered with ice, which is most probable, we must then leave our canoes, and trace the coast on foot to Hudson's Bay; or, if no North-West passage exists, to the shore which forms the boundary of Baffin's Bay. I think we are capable of executing this plan. Our chief dread was the hostile disposition of the Esquimaux. This danger is now almost obviated by the arrival of two Esquimaux interpreters, who have been provided at Churchill, and with great diligence sent after us.

"We are not so desolate, perhaps, in our exile, as our friends may suppose. The rein-deer are numerous about us, and we live on the most delicate venison. We find pleasure in the examination of a new and amiable race of people."

EGYPTIAN ANTIQUITIES.

On Friday Sept. 28th the celebrated alabaster Sarcophagus, which lately arrived from Alexandria, was uncased and deposited in the British Museum. It is for the present in one of the apartments not open to the public, where probably it will lie until a place is prepared for it in the Egyptian Gallery. This antique is certainly a very extraordinary and admirable specimen of the Arts of Egypt. The Sarcophagus is nine feet long, and about four feet high, apparently of a single piece, and that of a very fine alabaster. It is shaped like a modern coffin, and is more than large enough to hold the mummy with all its envelopes, which is presumed to have been deposited within this costly repository. But its chief value is in the innumerable hieroglyphics which cover the sides, interior and exterior, from top to bottom. They are small. The human figures, of which there are long processions in various circumstances and attitudes, erect, linked together, towing galleys, bending as if in worship, &c., are from an inch to an inch and a half high. Between those are compartments of symbols, the eye, the ibis, the lotus, &c. The serpent occurs frequently, and in some instances at considerable size, and with much exactness of detail. This noble work is supposed to be the coffin of *Psammis.* Conjecture, however, has an extensive range in Egyptian antiquity, and some [illegible] been suggested in favour of its being no tomb, but [illegible] a small shrine imitative of the original *Cymba*, or [illegible] vessel to which so many of the Indian emblems refer. The Ark seems to have formed a vast source of Pagan allegoric sculpture. The pecuniary value of this Sarcophagus has been estimated at a very large sum. It was the property of Mr. Salt, the British consul, and was, we understand, the subject of competition by the agents of some foreign powers.

The obelisk of red grani[illegible]rought home by the Dispatch, for Mr. Bankes jun., which li[illegible]n previously removed down the Nile from the island of Ph[illegible], on the borders of Nubia, has been safely unshipped at Deptford, and is now lying on the deck of the sheer-hulk there, till it is ready to be removed to Mr. Bankes's seat in Dorsetshire. It is particularly interesting, being the first ever brought to England. Artists have already been making drawings from it for the purpose of engraving; it being supposed that it may very possibly furnish a key to the interpretation of the hieroglyphical character; since the Greek upon the pedestal, which records its first erection, under Ptolemy and Cleopatra, near 2000 years ago, is very probably a translation of the hieroglyphics with which all the four sides of the obelisk itself are richly covered.

The celebrated Zodiac of Dendera, or Tentyra, which, when first discovered by the French during their expedition to Egypt, occasioned much discussion respecting the antiquity of the earth, has been lately brought to Marseilles, and is to be conveyed to Paris. The *Courier Français* states, that the English Consul in Egypt opposed its removal, on the ground that it was within the district in which he had purchased the right of digging for curiosities, and wished to claim it for his Government. The dispute was referred to the Pacha, who determined in favour of the French explorers, M. Saulnier and another. An account of their Journey is to be printed.

MERIDIANS OF GREENWICH AND PARIS.

On Tuesday, Sept. 25th, Captain Mudge, of the Royal Engineers (son of the late General Mudge), accompanied by M. Mathieu, Member of the Royal Institute at Paris, proceeded to Fairlight Downs, Hastings, and superintended the fixing of a vertical reflector, constructed by M. Mathieu, on the same spot selected by Gen. Roy 30 years since, to enable observations to be taken from the coast of France near Calais, for the purpose of re-measuring the distance between the meridian of the Observatories of Greenwich and Paris. The light from the reflector is visible at the distance of 90 miles; it consists of four circular wicks, the largest of which is 10 inches in circumference; it consumes two quarts of oil in the hour; it is lighted an hour before sun-rise and sun-set, and is kept burning for two hours. Capt. Mudge and M. Mathieu left Fairlight on the 24th ult. to proceed to join Major Colby and Capt. Kater in France.

AFRICAN GEOGRAPHY.

The following communication from Mr. Bowdich, the author of the "Mission to Ashantee," and other works on Africa, &c. will be interesting to our readers:

"I observe that the date of the *thirty*-FIRST of *April* occurs in Mungo Park's last journal, an error which has escaped the notice of his editor, as well as the correction of the traveller, who did not make an astronomical observation until the 17th of May, which, from the above cause, he calls the 16th, and consequently applied a wrong declination, as he continued to do in every subsequent observation. The consequence is, that the route is laid down considerably too much to the north; the latitude of Yamina, for instance, substituting the correct declination, is reduced from 13 deg. 15 min. N. to 12 deg. 52 min. N., and the important position of Sego, which was considered to be definitely settled, as regarded the latitude, must be lowered more than the third of a degree in all the subsequent maps of Africa."

SHIRT TREES.

" We saw on the slope of the Cerra Dnida," says M. Humboldt, " *shirt trees* fifty feet high. The Indians cut off cylindrical pieces two feet in diameter, from which they peel the red and fibrous bark, without making any longitudinal incision. This bark affords them a sort of garment, which resembles sacks of a very coarse texture, and without a seam. The upper opening serves for the head, and two lateral holes are cut to admit the arms. The natives wear these shirts of marima in the rainy season: they have the form of the *ponchos* and *ruanos* of cotton, which are so common in New Granada, at Quito, and in Peru. As in these climates the riches and beneficence of nature are regarded as the primary causes of the indolence of the inhabitants, the Missionaries do not fail to say in showing the shirts of *marima*, ' In the forests of the Oroonoko, garments are found ready made on the trees.' We may add to this tale of the shirts, the pointed caps, which the spathes of certain palm trees furnish, and which resemble coarse net work."

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Martin and Charles Grafton, of Birmingham, printing ink manufacturers, for their new method, of making fine light black of very superior colour, which for distinction from other blacks they called Spirit Black, and a new apparatus for producing the same.—Dated 24th October 1821.—2 months allowed to enrol specification.

To Benjamin Thompson, of Ayton Cottage, Durham, gent., for his method of facilitating the conveyance of carriages along iron and wood railways, tramways, and other roads.—24th October.—2 months.

To Charles Tuely the elder, of Kenton-street, Brunswick-square, Middlesex, cabinet-maker, for certain improvements applicable to window sashes either single or double, hung, fixed or sliding sashes, casements, window shutters and window blinds.—1st November.—6 months.

To Samuel Hobday, of Birmingham, patent snuffer maker, for his new and improved method or principle of manufacturing the furniture for umbrellas and parasols, and of uniting the same together.—1st November.—2 months.

To John Frederick Archbold, of Sergeants-Inn, Fleet-street, London, Esq. for his mode of ventilating close carriages.—1st November.—2 months.

To Richard Wright, of Mount Row, Kent Road, Surry, engineer,

gineer, for certain improvements in the process of distillation.—9th November.—6 months.

To David Redmund, of Agnes Circus, Old-Street-Road, Middlesex, engineer, for his improvements in the construction or manufacture of hinges for doors.—9th November.—6 months.

To Franz Anton Egells, of Britannia Terrace, City Road, Middlesex, engineer, for certain improvements on steam engines.—9th November —6 months.

To James Gardner, of Banbury, Oxfordshire, ironmonger, for his machine preparatory to melting in the manufacture of tallow, soap, and candles, and which machine may be used for other similar purposes.—9th November.—2 months.

To John Bates, of Bradford, Yorkshire, machine-maker, for certain machinery for the purpose of feeding furnaces of every description, steam engines, and other boilers, with coal, coke, and fuel of every kind.—9th November.—6 months.

To William Westley Richards, of Birmingham, gunmaker, for his improvement in the construction of gun and pistol locks.—10th November.—2 months.

To William Penrose, of Sturmmorgangs, Yorkshire, miller, for his various improvements in the machinery for propelling vessels, and in vessels so propelled.—10th November.—6 months.

To Edward Bowles Symes, of Lincoln's-Inn, Middlesex, Esq. for his expanding hydrostatic piston to resist the pressure of certain fluids, and slide easily in an imperfect cylinder.—10th November.—6 months.

To Joseph Grout, of Gutter-lane, Cheapside, London, crape-manufacturer, for his new manufacture of crape, which he conceives will be of great public utility.—13th November.—6 months.

To Neil Arnott, of Bedford-square, Middlesex, Doctor in Medicine, for his improvements connected with the production and agency of heat in furnaces, steam- and air-engines, distilling, evaporating, and brewing apparatus.—20th November —6 months.

To Richard Macnamara, of Canterbury-buildings, Lambeth, Surrey, Esq. for his improvement in paving, pitching, and covering streets, roads, and other places.—20th November.—6 months.

To John Collinge, of Lambeth, Surrey, engineer, for his improvements on hinges, which he conceives will be of public utility.—22nd November.—6 months.

To Henry Robinson Palmer, of Hackney, Middlesex, civil engineer, for his improvements in the construction of railways or trainroads, and of the carriage or carriages to be used thereon.—22nd November.—6 months.

Observations by Dr. BURNEY, at Gosport; the height of his Barometer being 50 feet above low-water mark.

Hour.	Barom.	Ther.			Wind.	State of the Weather.
1821. A.M. Nov. 12. 8h	Inches 29·94	52	50	96	S.W.	Sunshine and calm, with a *stratus*, and *cirrus* increasing from the westward, the latter modification forming a gray sky all the morning.
9	29·96	53	51	95	S.W.	Do. do. and some low passing *cirrostrati*.
10	29·99	54		93	S.W.	Nascent *cumuli* in conical and semicircular shapes.
11	30·00	56	55	88	S.W.	Dark horizontal streaks of *cirrostratus*, crossing the light tops of *cumulus* clouds.
12	30·02	59		82	S.W.	A faint solar *halo* 44° in diameter in the increasing *cirrus*, also passing *cumulostrati*.
P M 1	30·03	60	57	79	S.W.	A continuation of the *halo*, with plumose *cirrus*.

A thermometer placed on a level with the basin of my portable barometer, was all the morning 1¼° lower than the one attached to the top of the tube. I was induced to pay particular attention to this observation, from seeing it mentioned in your Philosophical Magazine and Journal for last June, p. 468, that "two thermometers, one suspended on each side and nearly in the centre of the barometrical tube, were 4 to 5° lower than the attached thermometer," which I thought was an extraordinary discrepancy in so short a space downwards. I have frequently tried this experiment within the last few months, but have never seen the thermometer placed in the middle of the tube, or on a level with the basin of my barometer, either in wet or dry weather, lower than 1½°, notwithstanding the accurate adjustment, and coincidence of the thermometers when placed together. Generally it is about 1° lower when placed level with the basin, in an airy room.

Since the 14th ultimo, 5.875 inches of rain have fallen here, making the quantity for this year up to the present time 32·365 inches, that is 6·615 inches more than fell in the neighbourhood last year.

The evaporation this year is comparatively small, being upwards of 12 inches less than the quantity of rain: therefore, the ground must be in a very moist state to a good depth.

The planet *Venus* was seen here with the naked eye this, and on the afternoon of the 5th instant, when on or near the meridian; and will become more visible in the open day, as she advances to her greatest elongation.

Nov. 14, 1821.

Leighton, Nov. 22, 1821.

Dear Sir,—I have the pleasure to send you the observations of the Barometer at this place and at Bushey, as usual, on 12th November.

LEIGHTON.

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·578	46	45	S.S. W.	calm.	Fine.
9	29·600	47	46	S .S.W.	do.	Do.
10	29·620	48	49	S .S.W.	do.	Cloudy.
11	29·633	48½	50	S.S.W.	do.	Do.
12	29·643	49½	52	S.S.W.	do.	Fine.
1	29·652	49¾	52	S.S.W.	do.	Do.

BUSHEY.

1821.	Barom.	Ther att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·361	50	48	W.S.W.	fresh.	Dense fog.
9	29·379	50	49	W.S.W.	do.	Cloudy.
10	29·399	50	50	S.W. by S.	moder.	Do.
11	29·417	51	51	W.S.W.	fresh.	Fine.
12	29·431	52½	52½	W.S.W.	do.	Do.
1	29·435	52·7	52·5	W. by S.	do.	Do.

The calculated height of Bushey above Leighton, by the observations made in October, by Colonel Beaufoy =212 1 feet.
by B. Bevan...... 211·
by the observations of this month.. 209·

My son Joseph has calculated the difference of the heights of Mr. Cary's instrument and mine for the last three months, as below, August .. .. 252· feet. London below Leighton.
September .. .. 254·
October .. .. 248·

Mean of 3 months 251·

I had some hopes of finding a section of the River Thames from London to the sea, either at the Trinity-House or at Guildhall; but am sorry to say, no such document is to be found at either place. When the commercial importance of the river is considered, and the great interest, as a matter of science, such a section would command, it is rather surprising that no public body or society have yet obtained this desirable information.

I am, dear sir, yours truly,

B. BEVAN.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Oct. 15	19	55·	30·	Fine—rain A.M.
16	20	52·	30·	Cloudy
17	21	53·	29·90	Fine—rain P.M.
18	22	57·	29·64	Cloudy
19	23	53·	29·55	Ditto
20	24	54·5	28·70	Rain—with brisk wind.
21	25	48·	29·	Fine
22	26	50·	28·95	Ditto
23	27	52·	29·15	Cloudy—rain P.M.
24	28	50·	29·26	Ditto
25	29	51·	29·65	Ditto—rain P.M.
26	new	56·	29·83	Ditto
27	1	56·5	29·87	Rain
28	2	59·5	30·	Cloudy
29	3	53·	29·95	Fine
30	4	53·	29·80	Ditto
31	5	56·	29·60	Ditto—foggy in the morning.
Nov. 1	6	57·	29·45	Cloudy
2	7	63·	29·37	Ditto
3	8	56.	29·37	Ditto—heavy rain P.M.
4	9	41·5	28·88	Rain and stormy
5	10	43·5	29·90	Fine
6	11	44·	30·15	Ditto
7	12	46·5	30·05	Cloudy
8	13	48·	30·05	Fine
9	full	48·	30·03	Ditto
10	15	48·	29·88	Cloudy
11	16	54·	29·10	Rain
12	17	51·	29·60	Fine
13	18	50·	29·13	Cloudy—foggy morning.
14	19	56·	29·48	Fine

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Oct. 27	54	59	55	30·23	Cloudy
28	55	62	46	·32	Cloudy
29	40	53	45	·29	Fair
30	42	52	46	·10	Fair
31	47	59	54	29·96	Fair
Nov. 1	56	60	57	·94	Fair
2	61	62	58	·93	Showery
3	55	53	47	·85	Rain
4	44	46	38	·36	Stormy
5	34	44	37	30·18	Fair
6	35	45	42	·35	Fair
7	43	50	47	·23	Fair
8	48	50	44	·21	Fair
9	4[illegible]	46	42	·18	Fair
10	40	50	50	·14	Foggy
11	50	56	50	29·91	Rain
12	50	55	47	·93	Fair
13	46	55	55	·90	Cloudy
14	55	56	54	·83	Cloudy
15	56	60	55	·72	Cloudy
16	55	50	50	·41	Stormy
17	50	54	50	·70	Rain
18	50	50	47	30·03	Fair
19	50	53	46	29·98	Rain
20	47	50	47	·93	Cloudy
21	50	48	40	·78	Showery
22	45	55	50	·51	Rain
23	50	47	39	·90	Showery
24	43	53	46	·70	Cloudy
25	47	47	50	·83	Rain
26	54	57	50	·27	Stormy

N B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the

12th Nov. 8 o'Clock M. Barom. 29 882 Ther. attached 52° Detached 50
— — 9 — — — — 890 — — — 52 — — 55
— — 10 — — — — ·918 — — — 53 — — 53
— — 1 — N. — — ·932 — — — 55 — — 55

LXXXII. *Description of an Appendage to* Tofft's *Blowpipe, to make it serve as a Substitute for* Brookes's *Gas Blowpipe. By Mr.* H. B. Leeson.

To Dr. Tilloch.

Nottingham, Nov. 16, 1821.

Sir, — The great danger attendant upon the burning the explosive mixture of oxygen and hydrogen in Brookes's Gas Blowpipe (even with Cumming's Safety Cylinder) has for some time been a source of regret to the votaries of science. Those who have witnessed the beautiful and brilliant effects produced by the gas blowpipe, and have considered the interesting nature of the facts that have been thereby developed, and the probable importance of the result of future experiments, must lament that any one should be debarred from using that powerful agent. The interposition of a screen may indeed prevent the fatal effects attending the explosion of the gases; but when the operator hears the alarming crash that announces the destruction of his apparatus, he must experience considerable disappointment at the loss of his blowpipe, and the disagreeable interruption of his experiments. The screen itself is an inconvenience, as it confines the operator to one particular spot, and requires much complicated apparatus, in order to allow the [illegible] syringe to be worked by a person on the outside of the screen. A desire to obviate these inconveniences led me some time since to contrive the safety appendages of which I now send you a description, as adapted to the improved hydraulic blowpipe described in No. 6, New Series of the "Annals of Philosophy." ABCD, fig. 1 (Pl. VI.) represent the body of the blowpipe, which should be about twenty inches long, six inches wide, and two feet deep, and may be made of tinned iron, well painted or japanned both inside and outside. There should be a lid to open on hinges at A, with a small hole in it to allow the top of the safety cistern to pass through it; this lid is not represented in the figure. The box is separated into two parts as represented by the dotted lines at B D; the lower part of the box is about eleven inches deep, and communicates with the upper part by the cylinder E: this cylinder is situated in the centre of the division B D; it is three inches in diameter, and reaches within half an inch of the bottom of the box. The air chamber F is supplied with gas or air through the pipe G, which should be about two feet long, one-fourth of an inch in diameter, and should be placed half an inch from the bottom of the box. The air issues through the pipe H, which should be about eleven inches long and the same diameter as the pipe G. To the top of the pipe H (which is strengthened by passing through the small shelf I) the safety appendages are screwed. There is a brass cock at K, the plug of which is worked by a key on the outside of the box. This cock is intended to regulate the flow of gas into the safety cistern at I, and also to prevent any water entering the pipe when the box is filled with water above the level of the division B D. When air or gas is introduced into the chamber F, previously filled with water through the pipe G, the air rises to the surface of the chamber, and expels the water through the cylinder E. The air cannot return through the pipe G, as the bottom of the pipe is closed by the water. There is a small cock or plug at L, in order to empty the water occasionally.

Figures 2 and 3 are sections of the safety appendages delineated of the real size. A B represents the cistern, which is 1¼ inch in depth, and half an inch in its internal diameter; when in use, this cistern must be filled with mercury as high as the dotted line at B. G is a bent pipe through which the gas enters the cistern A B; to the end of this bent pipe there is screwed a small valve F; this valve consists of a conical plug which fits its socket perfectly air tight; to the bottom of the plug there is attached a tube with one or more small holes drilled in the top of it. There is a small plate screwed round the bottom of the tube, in order to assist the opening of the valve by presenting a larger surface to the action of the air. The tube is fitted into a socket, which allows it to work easily up and down. The gas enters the tube at K, forces up the valve, and issues through the small holes below the plug, round which it passes into the bent pipe G. The valve must be made very true and light, and should have some small grooves at the top of the plug in order to afford a passage for the gas when the valve rises against the bottom of the bent pipe G. The section represents the valve lifted up as when in action. The lid C D screws into the cistern at A B; it is hollowed out in a conical form at D, and has the hole at E filled either with a piece of cane or wire-gauze. The gas which enters the cistern at H, below the surface of the mercury, collects in the conical part of the lid at D, and passes through the cane or wire-gauze at E into the jet pipe, which screws into the lid at C. The safety appendages are connected with the pipe H by a screw at the bottom of the valve as shown at I, fig. 1. When the cane or wire-gauze does not prevent the ignition of the gas in the safety cistern, the expansion of the gas forces the mercury up the bent pipe G, which falling on the plug shuts the valve, and closes all communication with the air chamber F, fig. 1. As soon as the ignition of the gas has ceased, the mercury returns into its place, and the gas flows through the cistern as at the commencement.

I shall now endeavour briefly to show the disadvantages attending the use of Brookes's gas blowpipe with Cumming's safety cylinder as at present adopted, and will then state how I conceive they are obviated in the one I have described. Brookes's blowpipe is obliged to have its sides made very thick and strong, in order to bear the condensation of the gases; consequently, when an explosion takes place, the sides are driven in all directions, like the fragments of a bomb, to the great danger of those near it; whereas, admitting the possibility of an explosion in the one now described, I know from experiment, that when the gases are exploded in a tin vessel similar to the body of the hydraulic blowpipe, the effect is simply to tear open the sides without separating them from the rest of the instrument. When an explosion takes place in Brookes's blowpipe, it is usually destroyed; whereas, owing to what has been before stated, the hydraulic blowpipe might generally be repaired at a trifling expense.

From the experiment above alluded to, I know that the water greatly deadens the force of the explosion; as the water is driven up the cylinder, which acts somewhat like a Welter's tube of safety, and would probably, could the sides of the box be made sufficiently strong, entirely prevent the bursting of the blowpipe.

As the condensation in Brookes's blowpipe diminishes, the flow of gas naturally becomes weaker, and this is probably the reason that the flame so frequently recedes. In addition to these inconveniences, the operator is interrupted every half minute by the necessity of replenishing the blowpipe with gas, and cannot without an assistant continue an experiment for any length of time. In the hydraulic blowpipe the whole of the gas is introduced at once; consequently there is no interruption of the experiments, and the gas is not contaminated by remaining in the bladder, which very much deteriorates it.

To fill the hydraulic blowpipe with gas, nothing more is necessary than to fill a bladder, screw it on the top of the pipe G, and squeeze the gas out with the hands. No condensing syringe is required for that purpose. This, and the power of completely exhausting the air chamber by filling it with water, renders the gas much less liable to be contaminated by atmospheric air.

In Cumming's safety cylinder the oil, or water, is constantly dropping through the valve at the bottom; so that, if used for any length of time, the whole of the oil or water escapes through the gas chamber, and leaves the cylinder completely empty; the great force by which the valve is opened in Cumming's safety cylinder, owing to the condensation of the gases, prevents it from closing properly when the gas within the cylinder is ignited, so that the expansion which then takes place (and which acts rather on the surface of the oil or water than upon the valve itself) drives the oil or water through the valve, and thus removes all obstructions to the further progress of the flame.

In the hydraulic blowpipe the pressure of the gas on the valve is comparatively trifling, so that the least counter pressure is sufficient to close it; the mercury cannot be driven through the valve, which is so constructed that it cannot be moved out of its vertical position, and the mercury must fall on the centre of the plug, which of course is immediately closed.

In Brookes's blowpipe there is a great waste of gas, as the last portion remaining in the chamber cannot be made use of, owing to the condensation ceasing; whereas in the hydraulic blowpipe the whole is forced out by the action of the water.

There is no occasion to fill the gas chamber with gas, as the water will force the whole out, be the quantity ever so small; and should the pressure of the water be found *too* weak, it is very easy to fill the blowpipe five or six inches above the level of the division A B, fig. 1; but care must be taken before putting in this additional quantity to close the cock at K, otherwise the water entering the pipe H would be driven up into the safety-cistern and occasion much inconvenience.

The safety appendages, if mercury be employed, must of course be made of iron; but those who prefer oil or water will find it easier to get them made of brass.

Those who choose it may easily use a screen with this blowpipe, by merely elongating the jet pipe, and they would have no need of any condensing syringe to be worked horizontally through the screen as in Brookes's, since the whole of the gas would be introduced into the gas chamber before the experiments commenced.

A small gauge to measure the quantity of air or gas in the chamber might be convenient, and could be easily formed by attaching a properly graduated rod to a cork float; and if a small hole were made in the top of the box for the rod to pass through, it would of course indicate by its rise and fall the quantity of air or gas in the chamber.

Should future experience confirm the opinion I have been induced to form of the safety of my appendages, we should have one instrument capable of producing every degree of heat from that requisite for roasting ores, bending glass, &c. to that necessary for the fusion of the most refractory bodies.

When this instrument is to be used as a common blowpipe, nothing more is necessary than to unscrew the safety appendages from the pipe H, and simply to screw the jet pipe in their place.

The hydraulic blowpipe might also be converted into an excellent gasholder for such gases as are unabsorbable by water.

Hoping you will excuse my trespassing so long on the time and patience of your readers, I remain, sir, yours most obediently,

H. B. LEESON.

P. S. As it is frequently desirable to have some means of preserving the products of experiment unexposed to the action of the atmosphere, the operator should procure some small glass tubes, open at one end but hermetically sealed at the other: when wanted for use, nothing more is necessary than to heat the tube over a spirit-lamp so as to expel the air, and then introducing the product of experiment, immediately to close the open end of the tube by hermetically sealing it.

The preceding Paper was accompanied by the following Letter from Mr. JOHN MURRAY.

To Dr. Tilloch.

SIR,—IN submitting to you Mr. Leeson's description of an appendage to Tofft's blowpipe, by means of which it may be made to substitute Brookes's instrument for the explosive atmosphere, I take leave to add, that when Mr. L. mentioned the idea to me, I suggested an *iron cylinder to contain mercury* instead of oil or water, on the plan adopted by the Marquis Ridolfi of Florence, and which I described in a former Number of the "Philosophical Magazine." With *Dr. Hope's Safety Wire-gauze Box,* I found it *quite safe,* charged with an explosive atmosphere; and I may here add, that with this attached to Brookes's blowpipe, I never had any explosion. I have used it *two years,* and without water or oil in the safety cistern.

I suggested the cylindrical double valve which Mr. Leeson has modified. The cane I mentioned as uniting all the advantages of a system of capillary tubes, and it is of considerable consequence to prevent explosion in the cylinder itself; for, if the receding explosive flame were suffered to acquire the momentum, it would thereby gain; the safety valve might be so injured as to give way altogether by the force of repeated subsequent explosions.

I have since fitted up the safety cistern with a *bundle of iron wires,* and, with the addition of a capillary pipe as a jet, I operate without the least danger.

I have deemed it proper to add these explanatory observations, and am, with much respect, sir, your obedient servant,

London. J. MURRAY.

LXXXIII. *Fur-*

LXXXIII. *Further Researches on the magnetic Phænomena produced by Electricity; with some new Experiments on the Properties of electrified Bodies in their Relations to conducting Powers and Temperature. By Sir* HUMPHRY DAVY, *Bart. P.R.S.**

I. IN my letter to Dr. Wollaston on the new facts discovered by M. Oersted, which the Society has done me the honour to publish, I mentioned, that I was not able to render a bar of steel magnetic by transmitting the electrical discharge across it through a tube filled with sulphuric acid; and I have likewise mentioned, that the electrical discharge passed across a piece of steel through air, rendered it less magnetic than when passed through a metallic wire; and I attributed the first circumstance to the sulphuric acid being too bad a conductor to transmit a sufficient quantity of electricity for the effect; and the second, to the electricity passing through air in a more diffused state than through metals.

To gain some distinct knowledge on the relations of the different conductors to the magnetism produced by electricity, I instituted a series of experiments, which led to very decisive results, and confirmed my first views.

II. I found that the magnetic phænomena were precisely the same, whether the electricity was small in quantity, and passing through good conductors of considerable magnitude; or, whether the conductors were so imperfect as to convey only a small quantity of electricity; and in both cases they were neither attractive of each other, nor of iron filings, and not affected by the magnet; and the only proof of their being magnetic, was their occasioning a certain small deviation of the magnetized needle.

Thus, a large piece of charcoal placed in the circuit of a very powerful battery, being a very bad conductor compared with the metals, would not affect the compass needle at all, unless it had a very large contact with the metallic part of the circuit; and if a small wire was made to touch it in the circuit only in a few points, that wire did not gain the power of attracting iron filings; though, when it was made to touch a surface of platinum foil coiled round the end of the charcoal, a slight effect of this kind was produced. And in a similar manner fused hydrate of potassa, one of the best of the imperfect conductors, could never be made to exert any attractive force on iron filings, nor could the smallest filaments of cotton moistened by solution of hydrate of potassa, placed in the circuit, be made to move by the magnet; nor did steel needles floating on cork on an electrized solution of this kind, placed in the Voltaic circuit, gain any polarity; and the only proof of the magnetic powers of electricity passing through such a fluid, was afforded by its effect upon the magnetized needle, when the metallic surfaces, plunged in the fluid, were of considerable extent. That the mobility of the parts of fluids did not interfere with their magnetic powers as developed by electricity, I proved, by electrifying mercury, and Newton's metal fused, in small tubes. These tubes, placed in a proper Voltaic circuit, attracted iron filings, and gave magnetic powers to needles; nor did any agitation of the mercury or metal within, either in consequence of mechanical motion or heat, alter or suspend their polarity.

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* From the Transactions of the Royal Society for 1821, Part II.

III. Imperfect conducting fluids do not give polarity to steel when electricity is passed through them; but electricity passed through air produces this effect. Reasoning on this phænomenon, and on the extreme mobility of the particles of air, I concluded, as M. Arago had likewise done from other considerations, that the Voltaic current in air would be affected by the magnet. I failed in my first trial, which I have referred to in a note to my former paper, and in other trials made since, by using too weak a magnet; but I have lately had complete success; and the experiment exhibits a very striking phænomenon.

Mr. Pepys having had the goodness to charge the great battery of the London Institution, consisting of two thousand double plates of zinc and copper, with a mixture of 1168 parts of water, 108 parts of nitrous acid, and 25 parts of sulphuric acid, the poles were connected by charcoal, so as to make an arc, or column of electrical light, varying in length from one to four inches, according to the state of rarefaction of the atmosphere in which it was produced; and a powerful magnet being presented to this arc or column, having its pole at a very acute angle to it, the arc, or column, was attracted or repelled with a rotatory motion, or made to revolve, by placing the poles in different positions, according to the same law as the electrified cylinders of platinum described in my last paper, being repelled when the negative pole was on the right hand by the north pole of the magnet, and attracted by the south pole, and *vice versâ*.

It was proved by several experiments that the motion depended entirely upon the magnetism, and not upon the electrical inductive power of the magnet; for masses of soft iron, or of other metals, produced no effect.

The electrical arc or column of flame was more easily affected by the magnet, and its motion was more rapid when it passed through dense than through rarefied air; and in this case, the conducting medium or chain of aëriform particles was much shorter.

I tried to gain similar results with currents of common electricity sent through flame, and *in vacuo*. They were always affected by the magnet; but it was not possible to obtain so decided a result as with Voltaic electricity, because the magnet itself became electrical by induction, and that whether it was insulated, or connected with the ground*.

IV. Metals, it is well known, readily transmit large quantities of electricity; and the obvious limit to the quantity which they are capable of transmitting seems to be their fusibility, or volatilization by the heat which electricity produces in its passage through bodies.

Now I had found in several experiments, that the intensity of this heat was connected with the nature of the medium by which the body was surrounded; thus a wire of platinum which was readily fused by transmitting the charge from a Voltaic battery in the exhausted receiver of an air pump, acquired in air a much lower degree of temperature. Reasoning on this circumstance, it occurred to me, that by placing wires in a medium much denser than air, such as ether, alcohol, oils, or water, I might enable them to transmit a much higher charge of electricity than they could convey without being destroyed in air; and thus not only gain some new results as to the magnetic states of such wires, but likewise, perhaps, determine the actual limits to the powers of different bodies to conduct electricity, and the relations of these powers.

A wire of platinum of $\frac{1}{220}$, of three inches in length, was fused in air, by being made to transmit the electricity of two batteries of ten zinc plates of four inches with double copper, strongly charged: a similar wire was placed in sulphuric ether, and the charge transmitted through it. It became surrounded by globules of gas; but no other change took place; and in this situation it bore the discharge from twelve batteries of the same kind, exhibiting the same phænomena. When only about an inch of it was heated by this high power in ether, it made the ether boil, and became white hot under the globules of vapour, and then rapidly decomposed the ether, but it did not fuse. When oil or water was substituted for the ether, the length of the wire remaining the same, it was partially covered with small globules of gas, but did not become red hot.

On trying the magnetic powers of this wire in water, they were found to be very great, and the quantity of iron filings that it attracted, was such as to form a cylinder round it of nearly the tenth of an inch in diameter.

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* I made several experiments on the effects of currents of electricity simultaneously passing through air in different states of rarefaction in the same and different directions, both from the Voltaic and common electrical batteries; but I could not establish the fact of their magnetic attractions or repulsions with regard to each other; which probably was owing to the impossibility of bringing them sufficiently near.

To ascertain whether short lengths of fine wire, prevented from fusing by being kept cool, transmitted the whole electricity of powerful Voltaic batteries, I made a second independent circuit from the ends of the battery with silver wires in water, so that the chemical decomposition of the water indicated a residuum of electricity in the battery. Operating in this way, I found that an inch of wire of platinum of $\frac{1}{220}$, kept cool by water, left a great residual charge of electricity in a combination of twelve batteries of the same kind as those above mentioned; and after making several trials, I found that it was barely adequate to discharge six batteries.

V. Having determined that there was a *limit* to the quantity of electricity which wires were capable of transmitting, it became easy to institute experiments on the different conducting powers of different met[illegible], and on the relation of this power to the temperature, [illegible] surface, or length of the conducting body, and to the conditions of electro-magnetic [illegible].

These experiments were made as nearly as possible [illegible] the same circumstances, the same connecting copper wire used in all cases, their diameter being more than an inch, and the contact being always preserved parts of the same solutions of acid and water were the different batteries, and the same silver wires and cuit with water were employed in the different trials; no globules of gas were observed upon the negative of the second circuit, it was concluded that the met ducting chain, or the primary circuit, was adequate to the discharge of the combination. To describe more minutely all the precautions observed, would be tedious to those persons who are accustomed to experiments with the Voltaic apparatus, and unintelligible to others; and after all, in researches of this nature, it is impossible to gain more than approximations to true results; for the gas disengaged upon the plates, the different distances of the connecting plates, and the slight difference of time in making the connexions, all interfere with their perfect accuracy.

The most remarkable general result that I obtained by these researches, and which I shall mention first, as it influences all the others, was, that *the conducting power of metallic bodies varied with the temperature, and was lower in some inverse ratio as the temperature was higher.*

Thus a wire of platinum of $\frac{1}{220}$, and three inches in length, when kept cool by oil, discharged the electricity of two batteries, or of twenty double plates; but when suffered to be heated by exposure in the air, it barely discharged one battery.

Whether the heat was occasioned by the electricity, or applied to it from some other source, the effect was the same. Thus, a wire of platinum, of such length and diameter as to discharge a combination without being considerably heated, when the flame of a spirit lamp was applied to it so as to make a part of it red hot, lost its power of discharging the whole electricity of the battery, as was shown by the disengagement of abundance of gas in the secondary circuit; which disengagement ceased as soon as the source of heat was withdrawn.

There are several modes of exhibiting this fact, so as to produce effects which, till they are witnessed, must almost appear impossible. Thus, let a fine wire of platinum of four or five inches in length be placed in a Voltaic circuit, so that the electricity passing through it may heat the whole of it to redness, and let the flame of a spirit lamp be applied to any part of it, so as to heat that part to whiteness, the rest of the wire will instantly become cooled below the point of visible ignition. For the converse of the experiment, let a piece of ice or a stream of cold air be applied to a part of the wire; the other parts will immediately become much hotter; and from a red, will rise to a white heat. The quantity of electricity that can pass through that part of the wire submitted to the changes of temperature, is so much smaller when it is hot than when it is cold, that the absolute temperature of the whole wire is diminished by heating a part of it, and, *vice versâ*, increased by cooling a part of it.

In comparing the conducting powers of different metals, I found much greater differences than I had expected. Thus, six inches of silver wire of $\frac{1}{220}$ discharged the whole of the electricity of sixty-five pair of plates of zinc and double copper made active by a mixture of about one part of nitric acid of commerce, and fifteen parts of water. Six inches of copper wire of the same diameter discharged the electricity of fifty-six pairs of the same combination, six inches of tin of the same diameter carried off that of twelve only, the same quantity of wire of platinum that of eleven, and of iron that of nine. Six inches of wire of lead of $\frac{1}{100}$ seemed equal in their conducting powers to the same length of copper wire of $\frac{1}{220}$. All the wires were kept as cool as possible by immersion in a basin of water *.

I made a number of experiments of the same kind, but the results were never precisely alike, though they sometimes approached very near each other. When the batteries were highly charged, so that the intensity of the electricity was higher, the differences were less between the best and worst conductors, and they were greater when the charge was extremely feeble. Thus, with a fresh charge of about one part of nitric acid, and nine parts of water, wires of $\frac{1}{220}$ of silver and platinum five inches long, discharged respectively the electricity of 30, and seven double plates.

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* Water is so bad a conductor, that in experiments of this kind its effects may be neglected altogether, and these effects were equal in all the experiments.

Finding that when different portions of the same wire plunged in a non-conducting fluid were connected with different parts of the same battery equally charged, their conducting powers appeared in the inverse ratio of their lengths; so, when six inches of wire of platinum of $\frac{1}{220}$ discharged the electricity of ten double plates, three inches discharged that of 20, 1½ inch that of 40, and one inch that of 60; it occurred to me that the conducting powers of the different metals might be more easily compared in this way, as it would be possible to make the contacts in less time than when the batteries were changed, and consequently with less variation in the charge.

Operating in this way, I ascertained that in discharging the electricity of 60 pairs of plates, one inch of platinum was equal to about six inches of silver, to 5½ inches of copper, to four of gold, to 3·8 of lead, to about $\frac{6}{10}$ of palladium, and $\frac{4}{10}$ of iron, all the metals being in a cooling fluid medium.

I found, as might have been expected, that the conducting power of a wire for electricity, in batteries of the size and number of plates just described, was nearly directly as the mass; thus, when a certain length of wire of platinum discharged one battery[*], the same length of wire of six times the weight discharged six batteries; and the effect was exactly the same, provided the wires were kept cool, whether the mass was a single wire, or composed of six of the smaller wires in contact with each other. This result alone showed, that surface had no relation to conducting power, at least for electricity of this kind, and it was more distinctly proved by a direct experiment: equal lengths and equal weights of wire of platinum, one round, and one flattened by being passed transversely through rollers so as to have six or seven times the surface, were compared as to conducting powers: the flattened wire was the best conductor in air from its greater cooling powers, but in water no difference could be perceived between them.

VI. I tried to make a comparison between the conducting powers of fluid menstrua and charcoal and those of metals. Six inches of platinum foil, an inch and 1-5th broad, were placed in

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[*] A foot of this wire weighed 1·13 grain, a foot of the other 6·7 grains.

a vessel which could be filled with any saline solution; and a similar piece of platinum placed opposite at an inch distance; the whole was then made part of a Voltaic circuit, which had likewise another termination by silver wires in water; and solution of salts added, till gas ceased to be liberated from the negative silver wire. In several trials of this kind it was found that the whole of the surface of six inches, even with the strongest solutions of common salt, was insufficent to carry off the electricity even of two pair of plates; and a strong solution of potassa carried off the electricity of three pair of plates only; whereas an inch of wire of platinum of $\frac{1}{220}$ (as has been stated) carried off all the electricity of 60 pair of plates. The gas liberated upon the surface of the metals when they are placed in fluids, renders it impossible to gain accurate results; but the conducting power of the best fluid conductors, it seems probable from these experiments, must be some hundreds of thousand times less than those of the worst metallic conductors.

A piece of well-burnt compact box-wood charcoal was placed in the circuit, being $\frac{3}{10}$ of an inch wide by $\frac{1}{10}$ thick, and connected with large surfaces of platinum. It was found that one inch and $\frac{4}{10}$ carried off the same quantity of electricity as six inches of wire of platinum of $\frac{1}{220}$.

VII. I made some experiments with the hope of ascertaining the exact change of ratio of the conducting powers dependent upon the change of the intensity and quantity of electricity; but I did not succeed in gaining any other than the general result, that the higher the intensity of the electricity, the less difficulty it had in passing through bad conductors; and several remarkable phænomena depend upon this circumstance.

Thus, in a battery where the quantity of the electricity is very great and the intensity very low, such as one composed of plates of zinc and copper, so arranged as to act only as single plates of from 20 to 30 feet of surface each, and charged by a weak mixture of acid and water. Charcoal made to touch only in a few points, is almost as much an insulating body as water, and cannot be ignited, nor can wires of platinum be heated when their diameter is less than $\frac{1}{80}$ of an inch, and their length three or four feet; and a foot of platinum wire of $\frac{1}{30}$ is scarcely heated by such a battery, whilst the same length of silver wire of the same diameter is made red hot; and the same lengths of thicker wires of platinum or iron are intensely heated.

The heat produced where electricity of considerable intensity is passed through conductors, must always interfere with the exact knowledge of the changes of their conducting powers, as is proved by the following experiment. A battery of 20 pair of plates of zinc, and copper plates 10 inches by 6, was very highly charged with a mixture of nitric acid and water, so as to exhibit a considerable intensity of electrical action, and the relative conducting powers of silver and platinum in air and water ascertained by means of it. In air, six inches of wire of platinum of $\frac{1}{70}$ discharged only four double plates, while six inches of silver wire of the same diameter discharged the whole combination: the platinum was strongly ignited in this experiment, whilst the silver was scarcely warm to the touch. On cooling the platinum wire by placing it in water, it was found to discharge 10 double plates. When the intensity of the electricity is very high, however, even the cooling powers of fluid media are of little avail: thus, I found that fine wire of platinum was fused by the discharge of a common electrical battery under water; so that the conducting power must always be diminished by the heat generated, in a greater proportion as the intensity of the electricity is higher.

It might at first view be supposed, that when a conductor placed in the circuit left a residuum of electricity in any battery, increase of the power of the battery, or of its surface, would not enable it to carry through any additional quantity. This, however, is far from being the case.

When saline solutions were placed in the circuit of a battery of 20 plates, though they discharged a very small quantity only of the electricity, when the troughs were only a quarter full, yet their chemical decomposition exhibited the fact of a much larger quantity passing through them, when the cells were filled with fluid.

And a similar circumstance occurred with respect to a wire of platinum, of such a length as to leave a considerable residuum in a battery when only half its surface was used; yet when the whole surface was employed, it became much hotter, and nevertheless left a still more considerable residuum.

VIII. I found long ago, that in increasing the number of alternations of similar plates, the quantity of electricity seemed to increase as the number, at least as far as it could be judged of by the effects of heat upon wires; but only within certain limits, beyond which the number appeared to diminish rather than increase the quantity. Thus, the two thousand double plates of the London Institution, when arranged as one battery, would not ignite so much wire as a single battery of ten plates with double copper.

It is not easy to explain this result. Does the intensity mark the rapidity of the motion of the electricity? or, merely its diminished attraction for the matter on which it acts? and does this attraction become less in proportion as the circuit, through which it passes, or in which it is generated, contains a greater number of alternations of bad conductors?

Mr. Children, in his account of the experiments made with his battery of large plates, has ingeniously referred the heat produced by the passage of electricity through conductors, to the resistance it meets with, and has supposed, what proves to be the fact, that the heat is in some inverse ratio to the conducting power. The greatest heat however is produced in air, where there is reason to suppose the least resistance; and as the presence of heat renders bodies worse conductors, another view may be taken, namely, that the excitation of heat occasions the imperfection of the conducting power. But till the causes of heat and of electricity are known, and of that peculiar constitution of matter which excites the one, and transmits or propagates the other, our reasoning on this subject must be inconclusive.

I found that when equal portions of wires of the same diameter, but of different metals, were connected together in the circuit of a powerful Voltaic battery, acting as two surfaces, the metals were heated in the following order: iron most, then palladium, then platinum, then tin, then zinc, then gold, then lead, then copper, and silver least of all. And from one experiment, in which similar wires of platinum and silver joined in the same circuit were placed in equal portions of oil, it appeared that the generation of heat was nearly inversely as their conducting power. Thus, the silver raised the temperature of the oil only four degrees, whilst the platinum raised it twenty-two. The same relations to heat seem to exist, whatever is the intensity of the electricity; thus, circuits of wires placed under water, and acted on by the common electrical discharge, were heated in the same order as by the Voltaic battery, as was shown by their relative fusion; thus, iron fusing before platinum, platinum before gold, and so on.

If a chain be made of wire of platinum and silver, in alternate links soldered together, the silver wire being four or five times the diameter of the platinum, and placed in a powerful Voltaic circuit, the silver links are not sensibly heated, whilst all those of the platinum become intensely and equally ignited. This is an important experiment for investigating the nature of *heat*. If heat be supposed a substance, it cannot be imagined to be expelled from the platinum; because an unlimited quantity may be generated from the same platinum, *i.e.* as long as the electricity is excited, or as often as it is renewed. Or if it be supposed to be identical with, or an element of, electricity, it ought to bear some relation to its quantity, and might be expected to be the same in *every* part of the chain, or greatest in those parts nearest the battery.

IX. The magnetism produced by electricity, though with the same conductors it increases with the heat, as I mentioned in my last paper; yet with different conductors I find it follows a very different law. Thus, when a chain is made of different conducting wires, and they are placed in the same circuit, they all exhibit equal magnetic powers, and take up equal quantities of iron filings. So that the magnetism seems directly as the quantity of electricity which they transmit. And when in a highly powerful Voltaic battery, wires of the same diameters and lengths, but of which the best conducting is incapable of wholly discharging the battery, are made, separately and successively, to form the circuit, they take up different quantities of iron filings, in some direct proportion to their conducting powers.

Thus. in one experiment, two inches of wire of $\frac{1}{30}$ of an inch being used, silver took up 32 grains, copper 24, platinum 11, and iron $8\frac{9}{10}$.

LXXXIV. *Account of an Hydraulic Orrery on an improved Principle of Motion.* By Mr. C. A. Busby.

To Dr. Tilloch.

Sir, — In pursuance of the suggestion of my much-respected friend, the venerable Dr. Hutton, I am induced to communicate to the public a short account of an improved principle of motion, adopted in an invention of mine, the hydraulic orrery, which has been so fortunate as to meet the particular approbation of many of our most distinguished philosophers.

About three years past I was engaged, during my stay at New-York, in a course of experiments to determine the resistance opposed to solid bodies of various forms in their passage through fluids. To perform these in the most simple and effectual manner, I provided a large circular bason or reservoir, and placed therein, near the circumference, any floating vessel that happened to be the subject of trial. This vessel was connected by an arbor to a floating centre, held in its place by a small shaft passing through it, and erected perpendicularly from the bottom of the reservoir. The bottom of the floating vessel was pierced, and a syphon, which it carried, being soldered into the aperture, rose from it, and extending over the circumference of the reservoir, its other extremity depended in air at a lower level than the surface of the water. This outer leg of the syphon was closed at the bottom; but a minute lateral aperture, resembling a very small finger-hole of a flute, being made, the water spouted through it (when the syphon was charged) in a direction parallel to the vessel, which instantly began to move with accelerated velocity in an opposite course. In a few seconds a maximum was attained, and the future progress exhibited that beautiful, continuous movement which can only find an adequate comparison in the silent gliding of the heavenly spheres. The idea instantaneously impressed me, and has been subsequently embodied with the most encouraging success in the novel machine above mentioned.

At present I have applied the principle, under appropriate modifications, no further than to the sun, the earth, and the moon, whose circuits, obliquities, parallelisms, and rotations, are displayed in apparently spontaneous movements on an area of five feet diameter. To effect these, four floating syphons are so combinedin succession, that a quantity of water equal to the discharge of a single stream about 1-6th of an inch diameter, with a head of seven inches, elicits every action. Each motion, as in nature, is perfectly independent; any one may be checked without impeding another; and when the hydraulic orrery commences its operations, it practically illustrates those incipient and gradually accelerating movements which may be supposed to have taken place within the mighty system itself, when, as in the beginning, the maximums of the greater motions were probably attained in succession.

This [illegible] principle (founded on Barker's mill, but now first combined with the syphon and applied to a floating body) is applicable to an extensive variety of experimental and philosophical purposes. It is so truly equable, that I make the novel and interesting experiment of producing a perfect hydro-parabolic mirror 54 inches diameter, and thus create any magnifying power *ad libitum*. Whirling tables on this principle will preserve any particular velocity during any required period of time, and the motion admits the most minute regulation, either by a variation in the length of the syphon, or of the size of the discharging aperture; or by so fixing a small flexible inclined plane to the syphon itself, and bending it into the stream, as that any proportion of its reaction may be neutralized by its action.

Another mean of obtaining an universal standard of measure is hereby provided independently of the pendulum. Thus, a given parabolic speculum will invariably be formed by any given rotation at any known level and latitude, and the focal distance of any parabola must under those circumstances be always a given dimension. A graduated revolving circle will also practically measure such minute portions of time as are beyond the recognition of the most accurate astronomical clocks. My orrery, when in action, lowers the surface of the water upon which it floats, about one inch in an hour; it is stopped merely by blowing air into the syphons, or by preventing the efflux of water in any other manner.

London, Nov. 1821. C. A. Busby.

P. S. I made the first, and comparatively imperfect, model of my hydraulic orrery at New-York, where it was seen in action by the Mathematical Professor Dr. Adrian, of Columbia College, Dr. David Hosac, F R.S., Dr. Samuel L. Mitchill, Dr Mac Neven, and many other leading members of the American Philosophical Society, established by the Legislature of the State in that beautiful and flourishing city. C. A. B.

LXXXIV. *On the rolling Pendulum.* *By* James Ivory, *M.A. F.R.S.*

It is proposed to demonstrate that the properties discovered by Huyghens, concerning the isochronous vibrations of a solid body about different fixt axes, are likewise true when the body rolls upon cylinders, provided the cylinders roll without sliding.

Let c denote the radius of the cylinder upon [illegible] the pendulum rolls; a, the distance of the axis of th[illegible] from the centre of gravity of the pendulum; ϕ the [illegible] plane passing through the axis of the cylinder and [illegible] gravity makes, at the time t, with a vertical pla[illegible] passing through the same axis. The whole mass of the body being m, let dm denote a molecule; x the distance of dm from the horizontal plane in which the axis of the cylinder moves; and y its distance from a fixt vertical plane, suppose that containing the axis of the cylinder and the centre of gravity when $\phi = 0$, or when the pendulum is at rest. For the sake of simplicity, we shall suppose that the whole matter of the pendulum is concentrated in a straight line; this supposition being made merely to abridge algebraic expressions; for nothing is easier than to extend what is proved in this case to a body of any figure.

Now, the accelerations impressed upon dm by its connection with the pendulum are $\frac{ddx}{dt^2}$ and $\frac{ddy}{dt^2}$, and the acceleration received from gravity is g; g denoting the velocity acquired by a falling body in one second: therefore, if δx and δy denote variations subject to the law of the motion of the molecule, we shall have this equation from the known principles of dynamics, viz.

$$S dm. \left\{ g - \frac{ddx}{dt^2} \right\} \delta x - S dm. \frac{ddy}{dt^2} \delta y = 0;$$

the symbol S denoting an integ[illegible] to be extended to all the molecules of the pendulum. The same formula will be more conveniently written thus,

$$Sdm. \left\{ \frac{ddx}{dt^2}\,\delta x + \frac{ddy}{dt^2}\,\delta y \right\} - S\,dm.\,g\,\delta x = 0.$$

Now, if r be the distance of dm from the axis of the cylinder, we have

$$x = r \cos\phi:$$

also, by the rolling of the cylinder, it is manifest that $c\phi$ is the distance of its axis from the fixt vertical plane; wherefore,

$$y = r \sin\phi - c\phi: \text{ consequently,}$$

$$\delta x = \delta\phi \times -r \sin\phi$$

$$\delta y = \delta\phi \times (r \cos\phi - c)$$

$$\frac{ddx}{dt^2} \qquad \frac{dd\phi}{dt^2}\, r \sin\phi - \frac{d\phi^2}{dt^2}\, r \cos\phi$$

$$\frac{ddy}{dt^2} \qquad \frac{dd\phi}{dt^2}\,(r \cos\phi - c) - \frac{d\phi}{dt^2}\, r \sin\phi:$$

and the preceding equation will therefore become

$$0: \frac{dd\phi}{dt^2}.\left(\int r^2 dm - 2c \cos\phi.\int r\,dm + c^2 m\right)$$

$$+ c\,\frac{d\phi^2}{dt^2}.\int r\,dm$$

$$+ g \sin\phi.\int r\,dm.$$

Now, by the nature of the centre of gravity,

$$\int r\,dm = ma:$$

and if we put mk^2 for the momentum of inertia of an axis passing through the centre of gravity parallel to the cylinder, then

$$\int r^2 dm = m(k^2 + a^2)$$

Hence, if we leave out m and put

$$h^2 = k^2 + (a - c)^2;$$

the foregoing equation will become

$$0 = \frac{dd\phi}{dt^2} \left\{ h^2 + 2ac(1 - \cos\phi) \right\} + ac\,\frac{d\phi^2}{dt^2} \sin\phi + ga \sin\phi.$$

In the case of very small vibrations, we may reject the terms above the first order; and then we get,

$$\frac{dd\phi}{dt^2} + \frac{ga}{h^2} \sin\phi = 0.$$

Now this equation, which is true of a body of any figure, belongs to a simple pendulum of the length $\frac{h^2}{a}$: and hence if τ denote the time of a complete oscillation of the rolling pendulum in a very small arc, we shall have

$$l = \frac{h^2}{a} = \frac{k^2 + c^2}{a} + a - 2c,$$

$\tau =$ [illegible]

π being the circumference of the circle of which the diameter is 1.

Again, let the same pendulum oscillate upon another cylinder, parallel to the first, and of the same radius with it, placed at the distance a' from the centre of gravity: then, τ' being the time of an oscillation, we have as before

$$l' = \frac{k^2+c^2}{a'} + a' - 2c$$

$$\tau' = \pi\sqrt{\frac{l'}{g}}.$$

If we suppose $\tau = \tau'$, then $l = l'$; and we get

$$l = \frac{k^2+c^2}{a} + a - 2c$$

$$l = \frac{k^2+c^2}{} + a' - 2c.$$

Subtract these equations and divide by $a-a'$; then

$$k^2 + c^2 = aa'. \qquad (1)$$

From this equation, $\frac{k^2+c^2}{a} = a'$, and $\frac{k^2+c^2}{a'} \quad a$; substitute these values in the expresions of l, and we obtain

$$l = a + a' - 2c. \qquad (2)$$

If we suppose $c = 0$; or, which is the same thing, if we suppose that the pendulum oscillates upon fixt axes, instead of rolling upon cylinders; the two foregoing equations will become

$$k^2 = aa'$$
$$l = a + a'$$

Now these last equations, which are familiar to geometers, comprehend all the properties of the isochronous oscillations of a body upon different fixt axes; and, by means of the equations (1) and (2), the like properties are extended to the case when the body rolls upon cylinders. It is to be observed that, in this investigation, it is not necessarily supposed that c, or the radius of the cylinder, is small: for the quantities left out in the general equation, were rejected on account of the smallness of the arc of vibration.

We have supposed that the cylinders are parallel; but the isochronism of the oscillations does not absolutely require this condition. When the axis passing through the centre of gravity is parallel to a given line, k^2 is determined in quantity; but it does not follow conversely that, when k^2 is given, the axis will have only one position. On the contrary, if we except the cases of a maximum or a minimum, there are many different axes (they will be all contained in a conic surface) that have the same momentum of inertia. The isochronism of the oscillations upon the two cylinders will take place when they are parallel to two axes possessed of equal momenta of inertia, and when their distances from the centre of gravity satisfy the equation (1). The sum of these distances diminished by twice the radius of the cylinder will then, by equation (2), give the length of the simple pendulum that oscillates in the same time.

Of all the infinite number of cases in which the same body may oscillate in the same time upon two equal cylinders, there is one, and one only, in which the length of the simple pendulum of isochronous vibration, is equal to the distance between the surfaces of the cylinders; and that is when the axes of the cylinders are parallel, and situated in the same plane with the centre of gravity. We may add that the equation (2) does not take place, unless the equation (1) be satisfied; which excludes an infinite number of cases of oscillation in equal times, when the cylinders are placed at equal distances from the centre of gravity, parallel to one another, or to axes that have equal momenta of inertia.

It is an indispensable in all that has been said, that the cylinders roll without sliding: the value of the ordinate y involves this condition.

J. Ivory.

Postscript.—Having, in the last Magazine, investigated the expression of the refraction in Cassini's hypothesis, it may be worth while to deduce from it a formula for the case when a star is not very near the horizon, for the sake of a comparison with the similar formula used in the construction of the French Tables. For this purpose nothing more is necessary than to expand the terms in the value of r, p. 344, retaining only the quantities multiplied by β, $i\beta$, β^2: thus we get

$$r = \beta \frac{\sin A}{\cos A} - i\beta \frac{\sin A}{\cos^3 A} + \tfrac{1}{2}\beta^2 \frac{\sin^3 A}{\cos^3 A},$$

or, $$r = \beta \tan A \left\{ 1 - \frac{2i - \beta}{2\cos^2 A} - \tfrac{1}{2}\beta \right\}.$$

The French formula, *Méc. Céleste*, vol. iv. p. 268, is this

$$r = \beta \tan A \left\{ 1 - \frac{2i - \beta}{2\cos^2 A} + \beta \right\};$$

which exceeds the first value of r by the quantity $\tfrac{3}{2}\beta^2 \tan A$. Now $\beta = {\cdot}0002938$; and hence

$$\tfrac{3}{2}\beta^2 \tan A = 0''{\cdot}027 \tan A.$$

At 70° from the zenith the difference will therefore be 0″·074; and at 80°, which is beyond the proper limit of the formulæ, it will amount to 0″·154. We are therefore warranted in saying that, when the same elementary quantities are used, there is, practically speaking, no difference in point of accuracy between the formula derived from the first and most simple hypothesis relating to the constitution of the atmosphere, and that obtained by the last effort of scientific skill; and this is a coincidence which it is surely both curious and instructive to mark.

J. I.

Erratum.—In the last Magazine, p. 342, 16th line from the top, the factor $\sin A$ is wanting in the value of r.

LXXXV. *Some Observations and Experiments on the Papyri found in the Ruins of Herculaneum. By Sir* Humphry Davy, *Bart. P.R.S.**

In a paper, intended for private circulation only, on the MSS. found in the excavations made at Herculaneum, but which was published, by mistake, in the Journal of Science and the Arts, I have described, in a general manner, the circumstances which led me to make experiments on these remains, and mentioned some of my first observations on this subject. Mr. Hamilton, to whom this communication was sent, entered into my views with all that ardour for promoting the progress of useful knowledge which so peculiarly belongs to his character; and on his representation of them, the Earl of Liverpool and Viscount Castlereagh, with the greatest liberality, placed at my disposal such funds as were requisite for paying the persons whom it was necessary to employ in trying new chemical methods of unrolling the MSS. and for examining and preserving them when unrolled; and His present Majesty, then Prince Regent, graciously condescended to patronize the undertaking.

In this communication, I shall do myself the honour of laying before the Royal Society an account of all that I have been able to do on this subject; namely, first, a detail of my early experiments in England on fragments of papyri, which induced me to believe that chemistry might afford considerable assistance towards unrolling the MSS. Secondly, a description of the rolls in the Museum at Naples, and of some analytical experiments I made upon them. Thirdly, a detail of the various chemical processes carried on in the Museum at Naples on the MSS., and of the reasons which induced me to renounce my undertaking before it was completed. And lastly, some general observations on the MSS. of the ancients.

I trust these matters will not be found wholly devoid of interest by the Society, and that they will excuse some repetitions of what I have stated in the Report before referred to, as they are necessary for a complete elucidation of the subject.

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* From the Transactions of the Royal Society for 1821, Part II.

1st. *An*

1st. *An Account of some Experiments made in England on Fragments of Papyri in* 1818.

In examining, chemically, some fragments of a roll of papyrus found at Herculaneum, the leaves of which adhered very strongly together, I found that it afforded, by exposure to heat, a considerable quantity of gaseous matter, which was principally inflammable gas, and when acted on by muriatic or nitric ether, it coloured them; and when it was exposed to heat after the action of these fluids, there was an evident separation of the leaves of the MS.

Chlorine and iodine, it is well known, have no action upon pure carbonaceous substances, and a strong attraction for hydrogen; and it occurred to me, that these bodies might with propriety be used in attempting to destroy the matter which caused the adhesion of the leaves, without the possibility of injuring the letters on the papyri, the ink of the ancients, as it is well known, being composed of charcoal.

Having through the polite assistance of Sir Thomas Tyrwhitt procured some fragments of papyri on which Dr. Sickler, and some on which Dr. Hayter had operated, and by the kindness of Dr. Young a small portion of a MS. which he had himself unsuccessfully tried to unroll, I made some experiments upon them, by exposing them to the action of chlorine and the vapour of iodine, heating them gently after the process. These trials all afforded more or less hopes of success. When a fragment of a brown MS. in which the layers were strongly adherent, was placed in an atmosphere of chlorine, there was an immediate action, the papyrus smoked and became yellow, and the letters appeared much more distinct; and by the application of heat the layers separated from each other, giving off fumes of muriatic acid. The vapour of iodine had a less distinct action, but still a sensible one; and it was found that by applying heat alone to a fragment in a close vessel filled with carbonic acid or the vapour of ether, so as to raise the heat very gradually, and as gradually to lower it, there was a marked improvement in its texture, and it was much more easily unrolled.

Even in these preliminary trials, I found that it was necessary to employ only a limited and small quantity of chlorine, too large a quantity injuring the texture of the layer, and decomposing the earths which it contained; and that the action of heat was much more efficacious when the MS. had previously been exposed to chlorine, as the muriatic acid vapour formed greatly assisted the separation of the leaves, and a smaller degree of heat was required. But in all the trials, I found the success absolutely depended upon the manner in which the temperature

perature was regulated. When the fragment was too rapidly heated, the elastic fluid disengaged usually burst the folds of the MS.; and when the heat was lowered too suddenly, the layers sometimes split in irregular parts, probably from the sudden contraction consequent on quick cooling.

From the products of the distillation of these fragments, which were water, acetous acid, ammonia, carbonic acid, and much inflammable gas, I inferred that the papyri to which they belonged must contain much undecomposed vegetable matter, and could not be purely carbonaceous; but as there were great differences in the appearances even of the few papyri in England, which had been presented to His Majesty George IV. when Prince of Wales, an opinion on this subject was more likely to be correct when formed after an examination not only of all the MSS. found at Herculaneum, but likewise of the circumstances of the excavations made there; and I had an opportunity, during the time I remained at Naples, in two successive winters, to satisfy my mind on this subject, and to obtain the information which will be given in the next section.

2dly. *On the State of the MSS. found at Herculaneum.*

The persons who have the care of the MSS. found at Herculaneum state that their original number was 1696, and that 431 have been operated upon or presented to foreign governments, so that 1265 ought to remain; but amongst these, by far the larger proportion are small fragments, or specimens so injured and mutilated that there is not the least chance of recovering any portion of their contents; and when I first examined the rolls in detail in January 1819, it did not appear to me that more than from 80 to 120 offered proper subjects for experiments; and this estimate, as my researches proceeded, appeared much too high. These MSS. had been objects of interest for nearly 70 years; the best had long ago been operated upon, and those remaining had not only undergone injuries from time, but likewise from other causes, such as transport, rude examination, and mutilations for the purpose of determining if they contained characters.

The appearances of different rolls were extremely various. They were of all shades of colours from a light chesnut brown to a deep black; some externally were of a glossy black, like jet, which the superintendants called "varnished;" several contained the umbilicus or rolling stick in the middle converted into dense charcoal. I saw two or three specimens of papyri which had the remains of characters on both sides, but in general one side only was written upon. In their texture they were as various as in their colours; the pale brown ones in general presented only a kind of skeleton of a leaf, in which the earthy matter was nearly in as large a proportion as the vegetable matter, and they were light, and the layers easily separated from each other. A number of darker brown ones, which, from a few characters discovered in opening them, appeared to be Latin MSS., were agglutinated as it were into one mass; and when they were opened by introducing a needle between the layers, spots or lines of charcoal appeared where the folds had been, as if the letters had been washed out by water, and the matter of which they were composed deposited on the folds. Amongst the black MSS. a very few fragments presented leaves which separated from each other with considerable facility, and such had been for the most part operated upon; but in general the MSS. of this class were hard, heavy, and coherent, and contained fine volcanic dust within their folds. Some few of the black and darker brown MSS., which were loose in their texture, were almost entirely decayed, and exhibited on their surface a quantity of brown powder.

The persons to whom the care of these MSS. is confided, or who have worked upon them, have always attributed these different appearances to the action of fire, more or less intense, according to the proximity of the lava, which has been imagined to have covered the part of the city in which they were found: but this idea is entirely erroneous, that part of Herculaneum being, as I satisfied myself by repeated examinations, under a bed of tufa formed of sand, volcanic ashes, stones, and dust, cemented by the operation of water (probably at the time of its action in a boiling state). And there is great reason to conclude, that the different states of the MSS. depend upon a gradual process of decomposition: the loose chesnut ones probably not having been wetted, but merely changed by the re-action of their elements, assisted by the operation of a small quantity of air; the black ones, which easily unroll, probably remained in a moist state without any percolation of water; and the dense ones, containing earthy matter, had probably been acted on by warm water, which not only carried into the folds earthy matter suspended in it, but likewise dissolved the starch and gluten used in preparing the papyrus and the glue of the ink, and distributed them through the substance of the MSS., and some of these rolls had probably been strongly compressed when moist in different positions.

The operation of fire is not at all necessary for producing such an imperfect carbonization of vegetable matter as that displayed by the MSS.: thus, at Pompeii, which was covered by a shower of ashes that must have been cold, as they fell at a distance of seven or eight miles from the crater of Vesuvius, the wood of the houses is uniformly found converted into charcoal; yet the colours on the walls, most of which would have been destroyed or altered by heat, are perfectly fresh, and where papyri have been found in these houses, they have appeared in the form of white ashes, as of burnt paper; an effect produced by the slow action of the air penetrating through the loose ashes, and which has been impeded or prevented in Herculaneum by the tufa, which, as it were, has hermetically sealed up the town, and prevented any decay, except such as occurs in the spontaneous decomposition of vegetable substances exposed to the limited operation of water and air; for instance, peat and Bovey coal.

The results of the action of heat upon the different specimens of the papyri, proved likewise, that they had never before been exposed to any considerable degree of temperature.

Various specimens of papyri were heated to dull redness in a small covered crucible of platinum to which air had no access. Some of the chesnut and most perfect specimens lost nearly half their weight, and the very black ones, and those containing the largest quantity of white ashes, all lost more than one-third, as the following results, selected from a numb[illegible]

No. 1. 100 parts of a pale chesnut papyrus, lost [illegible] parts.
No. 2. 100 parts of a decomposed papyrus, ches-
nut-coloured, but darker, lost [illegible]
No. 3. 100 parts of a very black papyrus, lost [illegible]
No. 4. 100 parts of a pale papyrus, extremely
loose in texture and partly converted into
white ashes, lost 41
No. 5. 100 parts of another of the same kind
lost 38

When the whole of the carbonaceous and vegetable matter of the papyrus was destroyed by slow combustion, the white ashes remaining, which were principally carbonate of lime and lime, proved to be from 1-16th to 1-20th of the original weight of the papyrus; and in those specimens which were most dense, and that contained a white powder, the proportion of ashes was greater, and a larger quantity was insoluble in acids.

Ammonia was found in the products of all the papyri that I distilled, but least in those which contained no distinct characters; from which it is probable that it arose principally from decomposed glue used in the manufacture of the ink, and which had been principally dissolved and carried off in those papyri which had been most exposed to the action of water.

I ascertained, that what the Neapolitans called varnish, was decomposed skin, that had been used to infold some of the papyri, and which by chemical changes had produced a brilliant animal carbonaceous substance; this substance afforded abundance of ammonia by distillation, and left ashes containing much phosphate of lime.

3dly. *An Account of the Experiments on Papyri made in the Museum at Naples.*

Only one method, and that a very simple mechanical one, has been adopted for unrolling the MSS. It was invented by Padre Piaggi, a Roman, and consists in attaching thin animal membrane by a solution of glue to the back of the MSS. and carefully elevating the layers by silk threads when the glue is dry.

In considering this method in its general application, some circumstances occurred to me which afforded an immediate improvement. A liquid solution of glue had been used, which, when the texture of the MSS. was loose or broken, penetrated through three or four layers, and these, when the glue dried, separated together. To obviate this objection, I mixed the solution of glue with a sufficient quantity of alcohol to gelatinize it; and a mixture of the jelly and the fluid being made and applied by a camel's hair brush, a film of jelly remained on the exterior of the surface of the leaf, which attached itself to the membrane.

The effect of the solution of glue applied in the ancient method, was always likewise to separate the layers, by expanding the imperfectly carbonized fibres. In the improvement I have mentioned, the alcohol, from its greater lightness, penetrated further into the papyrus, but produced its greatest effect immediately on the first layers.

I adopted in some cases ether, as an agent for assisting the separation of the layers; and it was always found very efficacious, whether it was necessary to remove a single layer, or several layers at a time, in order to discover if a roll contained characters. The ether was applied by a camel's hair brush lightly to the surface of the leaf, when its operation was intended to be merely on that leaf; and it was suffered to sink deeper according as more layers were to be separated; the mere circumstances of its evaporation, which in some cases I assisted by heat, tended to detach the layers. For the black MSS. I employed sulphuric ether, and for the brown ones muriatic or nitric ether in their impure states, *i. e.* mixed with much alcohol.

No artificial modes had been employed by the Neapolitans for drying the papyrus in the operation of attaching the membrane, and no means, except mechanical ones, of detaching it after it was dried.

By throwing a stream of air gradually warmed till it attained a temperature about that of boiling water upon the surface of the leaf, I succeeded not only in drying the layers with much greater rapidity, but likewise in separating them with more delicacy.

I tried different modes of heating the air to be thrown upon the papyrus, such as passing it in a spiral metallic tube through warm water or oil by a double bellows, and from a large bladder through a straight tube having a very fine orifice, and heated by a copper ball surrounding the body of the tube, and exposed to burning charcoal; which last method, from its simplicity, I found the one best fitted to the Neapolitan operators. By sending the stream of air from a greater or smaller distance, so that it mixed with more or less cold air, the degree of temperature applied was regulated at pleasure. It was always found necessary to suffer a few minutes to elapse after the membrane was attached, and then to begin with a very slight increase of temperature; as otherwise, by too sudden an application of heat, the membrane shrivelled before it became adherent, and the vapour suddenly raised destroyed its union with the papyrus; whereas, when the moisture was suffered to drain from the gelatinised glue, and the temperature was gradually raised, the expansion of the skin and the upper layer separated them perfectly from the lower layers, so that the unrolling was performed, as it were, by chemical means; and an operation, which hitherto had required some hours for its completion, was easily effected in from 30 to 40 minutes.

I tried several experiments, by substituting solution of resins in alcohol and of gums in water for the gelatinised solution; but none of them answered so well; the resins would not adhere with any tenacity to the membrane, and the gums, when dried, had not that flexibility which is an important character in the glue.

The alterations in the mode of applying and drying the membrane used to detach and preserve the leaves of MSS. capable of being unrolled, applied generally; I shall now mention the plans I adopted for the preparation of the MSS. for this operation.

MSS. in different states required a treatment of a directly opposite kind, which was to be modified according to circumstances. The pale chesnut-coloured MSS., covered partially with white ashes, were generally of a texture so loose, and had their layers so destroyed, that there was considerable danger of their falling into pieces by mere touching. The characters that remained in many of them were extremely distinct; and when a number of layers were taken up at once, it appeared as if they presented perfect columns of writing: but the fact is, the papyrus was full of holes, and each line was made up of letters from several different folds of the MS. When the process of unrolling these papyri was performed in the common way, the result obtained appeared, till it was examined minutely, a perfect column; but was in fact made up of the letters of different words. I endeavoured to obtain the fragments of a single leaf attached to a layer of membrane by applying a solution of caoutchouc in ether to the surface of a MS., so as to supply the parts of the leaf destroyed; but operating in this way, I obtained only a few characters, and never an entire word; so that, after various unsuccessful trials, I was obliged to give up the MSS. of this description as hopeless; more than 5-6ths of their contents probably being always destroyed, and that in so irregular a way as to leave no entire sentences, or even words.

On two brown MSS., which were firm in their texture, and had the appearance of peat, and the leaves of which would not separate by common means, I tried the experiment of heating, after they had absorbed a small quantity of chlorine; and I found that in both cases the leaves detached themselves from each other, and were easily unrolled: but these MSS. had been so penetrated by water, that there were only a few folds which contained words, and the letters were generally erased, and the charcoal which had composed them was deposited on the folds of the MSS.

Of the black MSS., of which the layers were perfect and easily separated, all the best specimens had been unrolled or operated upon, so that fragments only of this description remained. By assisting the operation of detaching the layers by muriatic ether and the other processes mentioned in page 126, many parts of columns were obtained from several of the fragments, by which some idea of their contents may be formed.

On the black compact and heavy MSS. which contained white earthy matter in their folds, I tried several experiments, with the hopes of separating them into single layers, both by the action of muriatic and nitric ether, and by the operation of chlorine and of weak hydrofluoric acid, assisted by heat; but generally the fibres of the papyrus had been so firmly cemented together, and so much earthy matter had penetrated them, that only a very imperfect separation could be obtained, and in parts where vestiges only of letters appeared, so that from MSS. of this kind only a few remains of sentences could be gained.

During the two months that I was actively employed in experiments on the papyri at Naples, I had succeeded, with the assistance of six of the persons attached to the Museum, and whom I had engaged for the purpose, in partially unrolling twenty-three MSS., from which fragments of writing were obtained,

tained, and in examining about 120 others, which afforded no hopes of success; and I should gladly have gone on with the undertaking, from the mere prospect of a possibility of discovering some better results, had not the labour, in itself difficult and unpleasant, been made more so by the conduct of the persons at the head of this department in the Museum. At first, every disposition was shown to promote my researches; for the papyri remaining unrolled were considered by them as incapable of affording any thing legible by the former methods, or, to use their own word, *disperati;* and the efficacy and use of the new processes were fully allowed by the Svolgatori or unrollers of the Museum; and I was for some time permitted to choose and operate upon the specimens at my own pleasure. When, however, the Reverend Peter Elmsley, whose zeal for the promotion of ancient literature brought him to Naples for the purpose of assisting in the undertaking, began to examine the fragments unrolled, a jealousy, with regard to his assistance, was immediately manifested; and obstacles, which the kind interference of Sir William A'Court was not always capable of removing, were soon opposed to the progress of our inquiries; and these obstacles were so multiplied, and made so vexatious towards the end of February, that we conceived it would be both a waste of the public money, and a compromise of our own characters, to proceed.

4thly. *Some general Observations.*

The Roman MSS. found in the Museum, are in general composed of papyrus of a much thicker texture than the Greek ones, and the Roman characters are usually larger, and the rolls much more voluminous; the characters of the Greek MSS., likewise, with a few exceptions, are more perfect than those of the Latin ones.

From the mixture of Greek characters in several fragments of Latin MSS., and from the form of the letters and the state of decomposition in which they are found, it is extremely probable that they were of a very ancient date when buried.

I looked in vain amongst the MSS. and on the animal charcoal surrounding them, for vestiges of letters in oxide of iron; and it would seem from these circumstances, as well as from the omission of any mention of such a substance by Pliny, that the Romans, up to his period, never used the *ink of galls and iron* for writing: and it is very probable, that the adoption of this ink, and the use of parchment, took place at the same time. For the ink composed of charcoal and solution of glue can scarcely be made to adhere to skin; whereas the free acid of the chemical ink partly dissolves the gelatine of the MSS., and the whole substance adheres as a mordant; and in some old parchments, the ink of which must have contained much free acid, the letters have, as it were, eaten through the skin, the effect being always most violent on the side of the parchment containing no animal oil.

The earliest MSS. probably in existence on parchment, are those codices rescripti, discovered by Monsignore Mai, in the libraries of Milan and Rome. Through his politeness I have examined these MSS., particularly that containing some of the books of *Cicero de Republica*, and which he refers to the second or third century. From the form of the columns, it is very probable that they were copied from a papyrus. The vegetable matter which rendered the oxide of iron black is entirely destroyed, but the peroxide of iron remains; and where it is not covered by the modern MSS., the form of the letter is sufficiently distinct. Monsignore Mai uses solution of galls for reviving the blackness. I have tried several substances for restoring colour to the letters in ancient MSS. The triple prussiate of potash, used in the manner recommended by the late Sir Charles Blagden, with the alternation of acid, I have found successful; but by making a weak solution of it with a small quantity of muriatic acid, and by applying them to the letters in their state of mixture with a camel's hair pencil, the results are still better.

It is remarkable, that no fragments of Greek, and very few only of Latin poetry, have been found in the whole collection of the MSS. of Herculaneum; and the sentences in the specimens we unrolled, in which Mr. Elmsley was able to find a sufficient number of words to infer their meaning, show that the works, of which they are the remains, were of the same kind as those before examined, and belonged to the schools of the Greek Epicurean philosophers and sophists.

Nearly 1000 columns of different works, a great part unrolled under the superintendance of Mr. Hayter, and at the expense of His present Majesty George IV., have been copied and engraved by the artists employed in the Museum; but from the characters of the persons charged with their publication, there is very little probability of their being, for many years, offered to the world; which is much to be regretted; for, though not interesting from their perfection as literary works, they would unquestionably throw much light upon the state of civilization, letters and science, of the age and country to which they belonged.

Should discoveries of MSS. at any future time be made at Herculaneum, it is to be hoped that the papyri will be immediately excluded from the atmosphere, by being put into air-tight cases, filled with carbonic acid after their introduction. There can be no doubt that the specimens now in the Museum were in a much better state when they were first discovered; and the most perfect even, and those the coarsest in their texture, must have been greatly injured during the 69 years that they have been exposed to the atmosphere. I found that a fragment of a brown MS. kept for a few weeks in a portion of air confined by mercury, had caused the disappearance of a considerable part of the oxygen, and the formation of much carbonic acid.

LXXXVI. *Remarks on Dr.* Reade's *Paper on Refraction. By Mr.* Charles Stark, *of Portsmouth.*

To Dr. Tilloch.

Sir, — In the Number of your Magazine for October, I observe a paper by Dr. Reade, on the subject of Refraction, wherein a very determined attempt seems to be made to overturn the whole doctrine of Dioptrics, and to explain all the phænomena of optics on the principle of reflection alone. If he should really succeed in the accomplishment of this design (which he seems to anticipate with no small degree of confidence), an important æra will, no doubt, be formed in the history of science, and an inevitable death blow given to those standard works on the subject, which have been so long adopted in our schools and universities. How far the Doctor is likely to succeed in effecting such a revolution, is my object here to inquire.

In the formation of any new theory, or in the determination of a general law in philosophy, such as the one under consideration, it may be presumed that the author, before publishing it to the world, would have observed the utmost degree of caution, not only in establishing the reasonableness of the hypothesis itself, but also in submitting it to the test of repeated and varied experiments, so as to be found not only consistent with itself, but successful in all its applications. In this respect, no theory has ever been employed, in any department of Natural Philosophy, with more complete success than that which Dr. R. is here endeavouring to explode. In reviewing the arguments, however, which he has brought forward in its refutation, and also those advanced in support of his own, it will require but little ingenuity of reasoning to show that his time and labour have been spent to very little purpose.

Dr. R. commences his paper by endeavouring to refute the explanation that is usually given of the common optical experiment of placing a piece of money at the bottom of an empty vessel, and its seeming to rise higher as water is poured into it. He objects to the common explanation by saying: "How can any bending of the rays of light bring the object nearer to the eye?" A satisfactory demonstration of the reason why it should appear so, may be found in almost every elementary treatise on optics; but from what he expresses in the next passage, it appears that it would require something else than either mathematical or [illegible] demonstration to satisfy him. "If," says he, "we bend a piece of iron wire, we certainly shorten the length it extended; but if the rays of light were so bent, they would fall short of the object!" What Dr. R.'s opinions may be of the manner in which light is transmitted, is not easy to guess; but I should suppose them to be quite as original as some of his other views: at all events, he certainly cannot suppose that it is either by a continued stream of particles from the luminous body, or by an agitation of the intervening medium, or he could never talk of comparing the rays of light to pieces of wire, or any other substance, of a determinate length. It therefore appears that his objection proceeds entirely from some peculiar metaphysical notions of the nature of light, and not from any absurdity which he can demonstrate to exist in either the reasoning or results of the old doctrine; and should he choose to communicate those ideas, we may have an opportunity of combating him with his own weapons.

But let us now proceed to what follows, where he pretends to have demonstrated by experiment the identity of reflection and refraction. The passage which it will be necessary to quote, is as follows:

"Having placed a piece of money at the bottom of a wine-glass, I made the edge of it intercept my view; on pouring in a small quantity of water, the shilling seemed to rise; I now perceived two images of the object, one at the bottom, and the other floating at the top of the water, very apparent when the glass was a little inclined to the eye. This floating image was agitated by every movement of the water. To ascertain whether this image was the real cause of vision, I held the glass above my eye, and saw the image floating by reflection on the surface of the water as if reflected from the face of a mirror. Further to convince myself that it is this floating image we see, and not the shilling at the bottom of the vessel, I brought my eye on a line with the image, and then gently lowering the glass, at the same time keeping my eye intently fixed on it, I saw the image by transmitted rays."

It happens rather unfortunately for Dr. R.'s doctrine, that this experiment, on which the whole of it is founded, is, of all others that he could possibly have hit upon, the best calculated to expose its absurdity. If he had only observed, in making the experiment, that when the glass was slightly shaken so as to agitate the surface of the water, the reflected image that was seen when the glass was placed *above*, would appear quite confused and indistinct, on account of the great dispersion of the reflected rays, while the other seen with the glass held *below* the eye would be comparatively little affected. Now this, which is explained in the most satisfactory manner by the laws of reflection and refraction, is quite irreconcileable with Dr. R.'s hypothesis; and it would be easy to point out abundance of other examples where it is equally inconsistent. What Dr. R. has observed with regard to the appearances of the two images, is nothing more than one of the many analogical relations that may be observed between the laws of refraction and reflection, but furnishes not the slightest proof of their identity.

As to what follows in the remaining part of the paper, it is merely an extension of the same principle which he deduces from the above experiment: consequently I do not, at present, consider it entitled to any further consideration.

I am, sir,
Your most obedient servant,

Portsmouth, Nov 13, 1821. CHA. STARK.

LXXXVII. *Thoughts on the Cultivation of Maize as a green Crop, to come in late in the Summer and Autumn.* By A PRACTICAL *and* EXPERIMENTAL FARMER.

IT is only in particular seasons, and in favourable situations, that Indian corn or Maize is ever known to ripen its seed in this climate, except by artificial means: every attempt, therefore, to naturalize and cultivate it, so as to produce a profitable crop of grain, will, it is to be feared, prove abortive. There is one purpose, however, for which we have reason to believe that its cultivation might be adopted by the English farmer, especially the cottage farmer, and with considerable advantages. I mean as an article of green food, in the place of spring tares or buckwheat. There are few annual plants of such rapid and luxuriant growth, and none by which it is exceeded, the sugar-cane excepted, in nutritious properties. So much, indeed, does it abound with saccharine matter, that it is no very uncommon practice, as I have been informed, in some parts of America to extract sugar from it. There are, it seems, several varieties of this plant, differing in the colour of the seed, their times of ripening, and in the luxuriance of their growth. It is unnecessary to say, that for the purpose for which it is here suggested, the last property ought certainly to be preferred, unless, indeed, it is slower in coming to maturity than the other varieties. The price of maize in the seed-shops is 1*s*. per quart. Its chief use in England is for feeding parrots. In the year 1798, 1799, and 1800, when Parkinson visited America, its price in that country was from 3*s*. to 5*s*. per bushel, when wheat was 11*s*. Were it once to be a regular article of importation for the purpose which I propose, it might be afforded at as easy a rate as buckwheat or spring tares.

In America, when the ears or cobs, as they are called, are ripe, they are cut and suspended [under cover we must suppose] to harden. The tops and blades [by which the stems, I conclude, are here meant] are cut, and kept to be given as hay in the winter to their cattle. Parkinson speaks of cutting the whole plant close by the ground, and afterwards reducing it into chaff as wanted. He does not state in what stage of its growth he cuts it; but we may presume when it is ripe or nearly so, as he afterwards adds, "the cobs, in this way of using them, not getting sufficient air to harden them, are apt to be mouldy, which cows did not dislike."

In proposing maize as a substitute for spring tares or buckwheat, it is on the supposition of its producing more abundantly than either of those articles, and of its affording more nutritious food: all this, however, must be proved by actual and accurate experiment, which, *Deo volente*, it soon shall be.

The above was written April 16, 1821 In the second week of the June following, this resolution was carried into effect by an experiment in my garden, on a very diminutive scale certainly, being only on three square yards. The result, however, has been more decisive than could have been expected with such limited data to go upon. The seed was sown promiscuously, and produced 126 plants, being 42 upon each square yard. In the second week of August I cut one stem, which I selected as being of average growth. It was then about three feet high, and weighed twelve ounces. I did not cut many more till the month of October, by which time some of them weighed five pounds. Had they been cut at the end of only two months from the time they were planted, I can have no doubt of their produce averaging after the rate of sixty tons per acre. I am equally confident that their weight in the month of October would not be so little as after the rate of twice that weight!

Having too small a quantity to try any conclusive experiment on larger cattle, I gave them to my pigs, of which they had an armful every day while they lasted; and though they were at that time fatting for the butcher in a grass field, with nearly as much corn and potatoes as they could eat, they yet devoured the green maize with the utmost avidity. Towards the latter end, when the stems of the maize began to get woody, they only champed the woody part so as to get the pith and sugary part out of it. Taking the average between 60 tons [the acreable produce in August], and 120 tons [the acreable produce in October], the mean produce would be 90 tons per acre, which no other crop, that I know of, could equal. I must here observe, that the ground on which the maize was raised, had every advantage that could be given to it; it was under a south hedge, and highly manured: to counterbalance these advantages, the soil is naturally a cold wet clay.

I have this autumn sown a patch of ground with winter tares. As soon as they are eaten off, I mean to follow them with maize, to be consumed in the same way. As I mean to keep an accurate register of the stock that is maintained by each crop, and of the time taken to consume it, I can easily ascertain their respective values. I could wish that two or three other persons would try the same experiment. But I do not, alas! recollect a single circumstance, from the first commencement of the Board of Agriculture to its final dissolution, to encourage me in expecting a wish so rational to be gratified!

A PRACTICAL and EXPERIMENTAL FARMER.

LXXXVIII. *Answers to "Questions addressed to Naturalists."* *By Mr.* GAVIN INGLIS.

To Dr Tilloch.

SIR, — IN your Magazine for September last, you have quoted from a German paper certain Questions addressed to Naturalists.

"The analysis of the earth (we are told) shows that it consists of the five following kinds: 1st, Calcareous: 2d, Quartz: 3d, Clay: 4th, Magnesia: 5th, Vegetable mould." It is then stated, that "it is affirmed that repeated experiments have proved that the first four, as well alone as intermixt, are absolutely unfruitful. If this be true, many thousand plants which now thrive only in vegetable mould could not grow on our earth some thousand years ago. Must we adopt the opinion that plants and vegetables have risen gradually? In East Friesland, if earths are dug up on the sea coast, &c from a depth of ten or twelve feet, plants then grow which are not otherwise to be met with in those parts of the country. Did those plants exist in the ancient world? Have their seeds retained the germinating power for some thousand years? Can that power be retained so long? Or, Whence do these plants come?"

Philosophical experiments, of whatsoever nature they be, should have one object in view, and only one—the discovery or elucidation of truth; and the experimenter should be possessed of the requisite knowledge and information, or arrange his proofs under the guidance of one more thoroughly conversant with the subject than himself. Had these experiments been skilfully conducted, no such conclusion could have been drawn. It would have been found that magnesian earth alone is inimical to vegetation, and that it detracts from the fertility of even the most fertile soils in proportion as its quantity bears to the mixture. Hence the comparative inferiority of all magnesian lime as a manure.

Many parlour experiments are undertaken, more with a view to give a gossamer support to some favourite hypothesis, than by a diligent investigation to prove the instability of a visionary theory. The mere theorizing philosopher is not the individual to conduct so interesting and delicate an inquiry: his ignorance of vegetable œconomy may give false results, from which he may draw the most erroneous conclusions. He who undertakes to elucidate this department of natural science, ought to be a practical horticulturist, a botanical amateur, thoroughly acquainted with the habits, œconomy, and habitat of every plant. If such an individual had been the conductor of these experiments, he would have proved that, although these earths supposed "*absolutely unfruitful*," either *per se* or mixed in any proportions, were not sufficiently fertile to give birth to plants that would flourish in garden soil, still they were capable of giving life to some more humble and hardy race, and that there was no reason for pronouncing such a doom. Before coming to such a conclusion, an intelligent mind has only to cast a thought over the globe, to be satisfied that every zone, as well as every soil, has plants exclusively its own.

As well may a theorizing philosopher, or parlour experimenter, look for the Lapland Lichen on the Libyan shores, or the great Aloe under the frozen snows of Nova Zembla, as expect that seeds the natives of rich and fertile soils should ever shew their germs in earths they scorn as beneath the pride of their families' luxuriance.

How would the West Indian smile at the grave tale of such experimenters, when viewing the now productive cultivated fields that but a few years before presented nothing to his forefathers' eye, but the hard unsubdued surface of a porous, calcareous rock? Even this as yet unpulverized calcareous earth was found under a skilful experimenter not absolutely unfruitful. But did the cultivator of this hitherto barren petrifaction ever think of planting on its obdurate surface his plantain, his corn, or his cane? No! Why? Because to *these* this rock would have been found "absolutely unfruitful." But into the pores of the rock this scientific cultivator insinuates the seeds of the Guinea grass.

The first rain that falls, fills the pores: the seeds drink up the moisture. They germ, and put forth their tender piles to receive strength and vigour from the solar rays, and prepare themselves to sip the dews of the evening, or drink deep of the next succeeding shower; while the roots indent deeper, and draw nourishment from the surface dust of this calcareous mass. The Guinea grass in time acquires strength and body sufficient for the planter's object. He then sets fire to the field and burns it down, reduces the whole dry foliage to ashes, which not only creates a quantity of vegetable earth, but at the same time produces a quantity of stimulating and fertilizing manure; while the fire calcines the exposed parts of the calcareous fragments, which pulverized by succeeding damps, and mixing with the ashes of the grass, adds to the accumulation of this newly created field. This operation is continued, and the burning repeated by the planter, till he has acquired a sufficient depth of soil for more profitable cropping. In like manner does the hardy Norwegian select patches to raise a little corn for his scanty pittance of bread. When nature has shed over the shelves and flats of his bleak inhospitable rocks, a quantity of the leaves or needles of the fir, sufficiently thick to retain moisture for the germination of seeds that fall in promiscuous profusion, the young firs spring forth thick as the matted turf. When these have attained a few years growth, in the autumn, preceding the corn crop, they are burnt down. The winter snows prepare the ashes for receiving the seed: on the first return of spring, the corn grows up, is cut down when ready, and the patch left for a succeeding crop of young firs, and again burnt, in regular rotation.

To be satisfied that quartz is not absolutely unfruitful, we have only to take a view of the sands along the sea-beaten shore, raised originally from the depths of the ocean and dashed on the beach by the surging waves, blown by stormy winds till imbanked beyond the rise of the highest tides. These sands are scarcely dried in the sun and washed by the vernal rains, when a race of vegetables peculiar to themselves bud forth and flourish, from seeds that may also have come from the ocean. These plants, by producing seed, propagate their species, and die. Their remains produce the first germs of vegetable mould, which in process of time accumulate, and may then be enriched by plants of a more luxuriant kind, and the soil become too effeminate for the coarse-grained aboriginal, whose existence may not now be traced, but whose seed lies safely imbedded and gone to rest. Disdaining the soil of the effeminate and voluptuous, they claim their primitive sand as a dormitory, ready to assume new life the moment they are left to the enjoyment of their native quartz.

In this manner soils gradually accumulate, and new vegetables appear, not by creation, nor the spontaneous effects of any admixture of the primitive earths, but from seeds the produce of former vegetable life; and to whatever depth this sand or soil may be buried, either by augmented accumulation or alluvial deposites, the seeds reposing in their native strata will remain for ever sound, and fit for bursting into new existence, when dug up from any depth, at any future period of the world, however remote.

Accumulating soils may be stored with new plants in a variety of ways, without having recourse to any supernatural production. The seeds of many plants are furnished with wings, and may be blown to a great distance. Birds may carry those that are destitute of the means of flying, and many a variety may be translated from one soil to another by the wanderings of cattle. Many of the winged tribes brought originally from the continent of America to enrich the botanical gardens of France, are now domiciled and scattered over and beyond the confines of Europe. When rivers are swoln with deluging floods, break in upon their banks, and carry to the sea the deposites of former times, the seeds of a very remote period may be thrown by the tempestuous dashing of the raging billows high upon the banks of a verdant shore, and give new life to a race of plants whose identity may have been extinct for many revolving ages. Even after the surface has acquired, by accumulation or otherwise, a fertility and richness of soil, such as to produce a covering of verdure sufficiently matted and interwoven to deny the intrusion of an aërial wandering variety, the mole comes in for his share of the general arrangement, and, by heaving up his mouldering heaps, interposes his aid in preparing a receptacle for the air-borne exotic, whose birth might be claimed by some very distant clime. Here a plant of another region bursts upon our notice—from whence, we know not till the scrutiny of the botanist retrace its flight to the land of its nativity.

Land gained from the waters of either the lake or the ocean, the deposite of rivers or of seas; the verge of gulfs, bays, or inlets, are the likeliest places in the world for the rise of strange plants, or the occurrence of such wonders as seem to astonish the natives of East Friesland. There is no country, however remote from the sea, or however elevated above its level, but communicates by its streams with the mighty waters of the ocean. There is no plant, however towering its situation, even those cresting the highest and most distant mountains, but whose seed may be blown or washed into some neighbouring stream, and carried uninjured along with its descending waters to the main, and wafted by the waves in a state of perfect preservation, to some very distant shore, and there find a root-bed and flourish, and in its turn shed its seed over the surrounding country; or find a preserving dormitory deep in the watery sediment, and remain in reserve for the incidental occurrences of revolving time. This sediment has been accumulating for ages on all the coasts of the Low Countries, and what was shore and surface some thousand years ago, may now be deep in the earth and far inland. This, however, does not prevent these former surfaces being dug up; and when dug up, nothing in nature can be more natural than the evolution of the native plants of the various strata, and even plants of very different and distant regions, whose seed may have been floated on the waters, or borne through the air on the wings of the storm. Vegetable productions of the West Indies and America have been floated across the expanded Atlantic, and thrown ashore on the beach of the Hebrides, the Orkneys, and Shetland: and very probably some of these may have found their way to the more southern shores of Europe, not excepting the coast of East Friesland.

The natural period of human existence is so very limited, as to preclude the possibility of watching the slow progressive operations of nature: the incident of to-day, may have had its embryo deep laid in nature a thousand years before its development. Our scanty knowledge of natural phænomena can only be gleaned from the few authenticated facts that are fortunately on record, and by calling to our aid the analogical reasonings that may be drawn from the wide field of material existence. In this contemplation, we soon perceive the perishable insignificance of the higher classes of animated nature, that a few revolving years lay prostrate in their kindred dust. But in the lower links of the animated chain, to the productions of the ovum, the duration and preservation of *latent* life are beyond the powers of our limited comprehension to calculate. The germs of vegetable existence I believe to be imperishable when bedded in their native strata, however deep. To the higher classes of the creation, to all who are endued with intelligence and power to protect and continue the existence of their kind, the principle and power of laying aside and resuming life has been denied;—while to the inferior orders, to the fly and to the reptile, either in the egg, the chrysalis, or the perfect animal, when bedded in earth beyond the influence of light, or inclosed in the solid rock, the revolution of a thousand years must be as one day, and one day as a thousand years; and when again called into new life, they are as capable of producing and propagating their kind, as when first laid to rest in their millesian dormitory.

However excentric, I am of opinion that the ocean has been, is, and will be the grand emporium and conservator of insect, reptile, and vegetable life. The rivers are daily running into the sea loaded with the spoils of the earth, carrying along with them the egg and chrysalis of the insect, the spawn of the reptile, with many of the reptiles themselves. These get imbedded in the calcareous and siliceous deposites of the waters. As this settles down, the animal keeps turning and moving; and as the soft substance begins to press upon its inmate, the creature, by pressure against the sides of the still impressive mass, *bakes* a bed, and keeps open sufficient space for itself, where it is destined to remain for time inconceivable. In the progressive revolutions of matter this mass consolidates, and petrifies around its prisoner, and in this state have the banished reptiles been found alive in the stratifications and formations of a former world. Here, the duration of latent life is beyond the extent of human knowledge.

The seeds of every plant of every region of the earth may be carried by the mountain torrents to the sea. Seed from the [illegible] pinnacle of the mountains that verge the western shores [illegible]thern America may be carried down the Amazonia, Rio [illegible] &c., and meet those of other tribes from the interior of Africa, on or in the South Atlantic; while the expanse of the North may be furnished by the river St. Lawrence, &c. Seeds from the mountains and shores of the Volga, the Danube, the Po and the Rhone, &c. may fill the Black Sea, the Archipelago, and the Northern Mediterranean, while the Nile fills up the African shores. The Western European rivers plenish the North and the German Oceans; the Ganges, &c. the Indian Seas: and there these seeds remain, in store and in entire preservation for the incidents of futurity.

Many proofs have been adduced of that portion of the globe which we inhabit, having at one period been the bottom of the ocean: if so, *vice versâ*, what was then inhabited and clothed in all the verdure of botanical glory, must now lie at the bottom of the mighty waters. In this case, had seeds of vegetables or eggs of vermes been perishable, all nature must have died, and the present world would have presented nothing but the bare surface of the water-worn rock and the noxious sludge and putrid deposites of by-gone ages, where food for neither man nor beast could have been found: nothing but one dreary waste of fearful extent would have covered the globe. Every maturized seed is an egg of the plant that produced it, and, like the animal ovum, contains within itself all the requisite powers and principles of evolving into life, of assuming the same organized symmetry of its parent plant, and of reproducing its kind. In nothing does the protecting hand of Providence show solicitude more conspicuously than in the formation of the seeds and preservation of vegetable life; every seed is covered with a shell—a coat of mail to shield it from external injury; and as the vegetable egg is left without the fostering care of a watchful parent, its shell has the power of absorbing and giving out moisture, of dilating and contracting as the state of its charge may require; and although originally a mere vegetable pulp, yet its constituent principles are furnished with powers of defence, capable of resisting, uninjured, the utmost effects of animal digestion, even of those animals whose gastric energies are sufficient to dissolve the hardest bones. In proof of this, I may state two instances that must be familiar to every one. Pease fully ripened, and allowed to harden in the pod, if swallowed whole, will defy the strongest digestive powers, pass through the intes[illegible]d grow. Let the hardened pease be boiled till the fari[illegible]educed to pulp, still the shell of the pea will remain undis[illegible] and although taken into the stomach, with all this previous preparation of boiling, the shell will yet resist the utmost efforts of animal digestion, and pass through the intestines unaltered. Thin as the bran of wheat may be, that shell is sufficient to defend the grain against the digestive powers of the horse. When taken unbruised into the stomach of that noble animal, the grain passes through his entrails, not only uninjured and fit for vegetation, but will be found to have absorbed a fertilizing principle, and to produce more luxuriant foliage, and better grain, than when sown in its natural state. If seeds resist animal digestion, the natives of East Friesland and the Querists may cease to wonder at their dormancy for any length of time. Not to repeat any of my notes (see Phil. Mag. for December 1818), I may be allowed to state another instance of seeds retaining their germinating principle, that has since occurred.

On clearing out the *Monk barns* of an old religious establishment in the North, and subjecting the site to the operations of the plough, several varieties of oats sprung up the succeeding spring, that have long been unknown in the country, whose seed must have been carried by mice or rats, &c. deep into their burrows as winter store. The collectors of this latent repository may have perished by the cats, the weasels, or the *traps* of the Holy Fathers, and their little store remained unconsumed. These oats must have retained their vegetating powers (since the days of *John o' the Girnal*[*]) at least for some hundred years. If for hundreds, why not for thousands? and if for thousands, why not for ever?!!

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[*] See The

Many of my observations and ideas may appear prolix and trifling, yet I cannot resist bringing forward whatever may assist in elucidating a subject I consider so very interesting. Well may we say with the Spectator, "that to enjoy the world is to know it; and to have just conceptions of our Almighty Creator and Preserver, is to trace the infinite greatness and wisdom of creative power, and the unbounded intelligence and design so conspicuously displayed in protecting and preserving the work of his grace."

Nov. 12, 1821. GAVIN INGLIS.

LXXXIX. *On Mr.* SOUTH'S *Catalogue of Double Stars.*

By A CORRESPONDENT.

To Dr. Tilloch.

SIR, — I AM a man fond of science, but, having received only a plain education, have been obliged to work my way by my own exertions, and such helps as I have been fortunate enough to meet with.

I shall ever remember with gratitude the obligations I am under to your and other periodical works, by whose kind assistance I have been enabled to ask, and have generally received, such information as I at times stood in need of, and without which I never could have surmounted the many difficulties which opposed my progress. Believe me, it is only persons like me, who have been obliged to fight their way through the difficult paths that lead to mathematical knowledge,—it is only such, I say,—that can appreciate the utility of such publications. But, my gratitude is leading me from my object, which, as aforetime, is to ask assistance. Chance, some time ago, threw in my way a Catalogue of Double Stars, presented by J. South, Esq. to the Astronomical Society. Their places, as the learned author informs us, are taken from Bode's Catalogue, but reduced by him from the year 1801 to that of 1821. I was acquainted (at least I thought so) with the method of making these reductions; but as I had never attempted it, I thought it a good opportunity to put my knowledge to the test, by trying the reduction of a few of these stars, and was much pleased to find the declinations, with which I first began, to coincide with the numbers in the table before me:—but judge my astonishment, when I found scarce a single right ascension to agree. I looked over my calculation again and again; I re-perused Dr. Maskelyne's rule, re-calculated the annual variations; and, to be certain that I had made no mistake, I set my son, a clever little fellow of ten years of age, to work the calculations, as an arithmetical exercise, explaining to him, as well as I could, the object to be obtained. All this he perfectly well understood, except the reducing the seconds of a degree into seconds and tenths of time (the right ascension being given in space by Bode, but in time by Mr. South). This I told him he must leave for me, as it required some little knowledge of decimals, which he had not yet learned. My boy was much flattered in being thought worthy to attempt what he had seen his father so long puzzling at in vain; but I can never forget his exultation and triumph, when he put into my hand his calculations, all agreeing with Mr. South's Catalogue. "Why how," I exclaimed, "have you done it?" "Done it!" he replied: "Why, those plaguing seconds of a degree which I could make nothing of, and which I believe no one else can make anything of, I kicked out; and then going on with the calculation as you told me, it all came right, and I have no doubt this is the way the gentleman does it." I endeavoured to repress his unfounded exultation, by informing him that this Catalogue was the production of a gentleman of great mathematical abilities, a member of our most learned Societies, and the author of many papers on mathematical subjects of the most profound nature; that in these calculations I had observed he had had regard to even hundredths of a second; and therefore, to say that such a gentleman would disregard twenty or thirty seconds of a degree, or would find any difficulty in reducing them into time, was talking nonsense, or rather like a child as he was, who knew nothing about the matter. My boy, however, has a will of his own, and insists upon maintaining that his method is right, unless I can show him any other that will solve the difficulty. This I own I cannot do; and, to be serious, shall be much obliged to the learned author, or any other of your correspondents, if they can inform me how these right ascensions were calculated, or whether any improvement has recently been made in the rule given by Dr. Maskelyne for the reduction of stars from one epoch to another.

Excuse this garrulous epistle from an old man, and extend the like indulgence to the son of his old age, whom with the partiality of a fond father he has taken the liberty to introduce; a liberty which he would not have presumed upon, but to show how, by a fortuitous circumstance almost beyond credit, the empirical reasoning of a child should lead to the same results as the profound calculations of a learned mathematician.

I am, sir,
Your very obedient and obliged servant,
Z.

XC. *Notices respecting New Books.*

Recent Publications.

The Philosophical Transactions of the Royal Society of London for 1821, Part II. are just published, and the following are their contents:

XIV. An Account of Experiments to determine the Times of Vibration of the Pendulum in different Latitudes. By Captain Edward Sabine, of the Royal Reg. of Artillery, F.R.S. and F.L.S. —XV. Some Observations and Experiments on the Papyri found in the Ruins of Herculaneum. By Sir Humphry Davy, Bart. P.R.S.—XVI. Observations on Naphthaline, a peculiar Substance resembling a concrete essential Oil, which is apparently produced during the Decomposition of Coal Tar by Exposure to a red Heat. By J. Kidd, M.D. Professor of Chemistry, Oxford. Communicated by W. H. Wollaston, M.D. V.P. R.S.—XVII. On the Aberrations of compound Lenses and Object-Glasses. By J. F. W. Herschel, Esq. F.R.S. —XVIII. An Account of the Skeletons of the Dugong, two-horned Rhinoceros, and Tapir of Sumatra, sent to England by Sir Thomas Stamford Raffles, Governor of Bencoolen. By Sir Everard Home, Bart. V.P.R.S.—XIX. On the mean Density of the Earth. By Dr. Charles Hutton, F.R.S.—XX. On the Separation of Iron from other Metals. By J. F. W. Herschel, Esq. F.R S. — XXI. On the Re-establishment of a Canal in the Place of a Portion of the Urethra which had been destroyed. By Henry Earle, Esq. Surgeon to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital. Communicated by the President.— XXII. Calculations of some Observations of the Solar Eclipse on the 7th of September 1820. By Mr. Charles Rumker. Communicated by Thomas Young, M.D. For. Sec. R.S.—XXIII. An Account of the Re-measurement of the Cube, Cylinder, and Sphere, used by the late Sir George Shuckburgh Evelyn, in his Inquiries respecting a Standard of Weights and Measures. By Captain Henry Kater, F.R.S.—XXIV. An Account of Observations made with the Eight-feet Astronomical Circle, at the Observatory of Trinity College, Dublin since the Beginning of the Year 1818, for investigating the Effects of Parallax and Aberration on the Places of certain fixed Stars; also the Comparison of these with former Observations for determining the Effects of Lunar Nutation. By the Rev. John Brinkley, D.D. F.R.S. and M.R.I.A. Andrews Professor of Astronomy in the University of Dublin.—XXV. On the Effects produced in the Rates of Chronometers by the Proximity of Masses of Iron. By Peter Barlow, Esq. of the Royal Military Academy. Communicated by John Barrow, Esq. F.R.S.—XXVI. On the Peculiarities that distinguish the Manatee of the West Indies from the Dugong of the East Indian Seas. By Sir Everard Home, Bart. V.P.R.S.—XXVII. On a new Compound of Chlorine and Carbon. By Richard Phillips, F.R.S.E. F.L.S., and Michael Faraday, Chemical Assistant in the Royal Institution. Communicated by the President.—XXVIII. On the Nerves; giving an Account of some Experiments on their Structure and Functions, which lead to a new Arrangement of the System. By Charles Bell, Esq. Communicated by the President.—XXIX. Further Researches on the magnetic Phænomena produced by Electricity; with some new Experiments on the Properties of electrified Bodies in their Relations to conducting Powers and Temperature. By Sir Humphry Davy, Bart. P.R.S.

A Voyage to Africa, including a Narrative of an Embassy to one of the interior Kingdoms, in 1820; with Remarks on the Course and Termination of the Niger, &c. By Wm. Hutton, Esq. late Acting Consul for Ashantee. Illustrated with Maps and Plates. 8vo. pp. 488.

A practical Treatise on the Sliding Rule; in Two Parts.—Part the First, being an Introduction to the Use of the Rule generally, as adapted for Calculations that usually occur to Persons in Trade.—Part the Second, containing Formulæ for the Use of Surveyors, Architects, Civil Engineers, Scientific Gentlemen, and for Schools in general. By B. Bevan, Civil Engineer and Architect. 1 vol. 8vo. 6*s*.

Amusing Experiments for Young People. No. 1. Price 1*s*. to be completed in six, with numerous Engravings. Upwards of Five Hundred New and Amusing Experiments for Young People; with Observations on the Substances employed, and their Application to useful Purposes. By George G. Carey, Lecturer on Chemistry and Experimental Philosophy.

Part I. of Views of the Colosseum; engraved, by W. B. Cooke and J. C. Allen, from Drawings of Major Cockburn. In this Work will be displayed the stupendous Proportions and picturesque Beauties of the Colosseum, that interesting Ornament of Ancient Rome. It will be completed in 5 Parts, containing 15 Line Engravings, together with Plans, Sections, and Elevations, and a descriptive History of the Building. Super-royal folio. 1*l*. 1*s*. each Part, Proofs 1*l*. 10*s*.

A System of Pathological and Operative Surgery, founded on Anatomy, illustrated by Drawings of diseased Structure, and Plans of Operation. By Robert Allan, F.R.S. &c. 8vo. Vols. I. and II. 12*s*. 6*d*. each, bds.

A Compendious Treatise on the Theory and Solution of Cubic and Biquadratic Equations, and of Equations of the higher Order. By the Rev. B. Bridge, B.D. F.R.S. 8vo. 6*s*. bds.

Twelve Essays on the proximate Causes of the material Phenomena of the Universe, with illustrative Notes. By Sir Richard Phillips. 1 vol. 12mo.

Just published, An Epitome of Pharmaceutical Chemistry; whereby the Art of prescribing scientifically may be facilitated, and those Decompositions avoided, which, resulting from Combinations of incompatible Substances, often frustrate the Views of the Practitioner in their Medical Effects; arranged according to the London Pharmacopœia. By Rees Price, M.D. Member of the Royal College of Surgeons in London; Honorary Member of the Medical and Physical Society of Guy's Hospital, &c. 12mo. 3*s*.; or, on a Chart adapted for framing, 2*s*. 6*d*.

Just imported, (dedicated by Permission to the Most Noble the Marquis of Hastings,) a Grammar of the Sunscrit Language, on a New Plan. By the Rev. William Yates. 1 vol. Demy 8vo. 2*l*. 10*s*. Royal, 4*l*.

Preparing for Publication.

Two Voyages to New South Wales and Van Diemen's Land, by Thomas Reid, Surgeon in the Royal Navy, 1 vol. 8vo., will appear with the New Year.

The *Encyclopædia Metropolitana*, the publication of which had been suspended, having become the right of new Proprietors, will soon be resumed, and carried on with spirit.

History of Cultivated Vegetables, comprising their Botanical, Medicinal, Edible and Chemical Qualities, Natural History, and Relation to Art, Science, and Commerce. By Henry Phillips, Author of *Pomarium Britannicum*; or, A History of Fruits known in Great Britain. The Work to be printed in 2 vols. royal octavo, price to Subscribers 1*l*. 11*s*. 6*d*. bds.

Travels through Africa, from Egypt to the Cape of Good Hope. By Mr. Waldeck, a German, who has recently arrived in England from India. This journey is, it seems, no fiction. It appears that at the foot of the Mountains of the Moon he found an inscribed pillar erected by a Roman Consul about the period of the reign of Vespasian. He found a level on the top of those mountains nearly 400 miles broad, on which he discovered a Temple of the highest antiquity, and in fine preservation, and still used for religious purposes by the inhabitants. South of the level, he passed a descent of fifty-two days journey; and when advanced about nine days, he found the skeleton of a man with a telescope slung on his shoulder marked with the name of Harris, and also a chronometer made by Marchand. There were two other skeletons, and it was supposed the owners perished for want of water. The manuscript is preparing, and the work will speedily appear in London, accompanied by engravings. Mr. Waldeck was accompanied by four European companions, only one of whom survived the hardships of the journey, and now resides in Paris.

The Second Volume of Sir R. K. Porter's Travels in Georgia, Persia, Armenia, Ancient Babylonia, &c. &c. It will be illustrated with numerous Engravings of Portraits, Costumes, Antiquities, &c.

An interesting Volume of Travels will appear shortly by W. J. Burchell, Esq. whose Researches in the Interior of Southern Africa, during a five Years' Residence in that Country, comprise a Variety of Discoveries and Observations which have never yet been laid before the Public. Numerous Engravings, from the Author's own Drawings, and an entirely new Map, will illustrate the Work.

Mr. A. T. Thomson, F.L.S. &c. &c. has in the Press Lectures on the Elements of Botany. Part I. containing the Anatomy and Physiology of those Organs on which the Growth and Preservation of the Plant depend: with Explanations of the Terminology connected with these Parts: 8vo. illustrated by marginal Cuts and Copper Plates.

Shortly will be published, Practical Observations on Paralytic Affections, St. Vitus's Dance, Distortions of the Spine, and Deformities of the Chest and Limbs, arising from Chronic Rheumatism, Rickets, Gout, &c. illustrative of the beneficial Effects of Muscular Action: with Cases: by W. Tilleard Ward, F.L.S.

In a few weeks will be published, An Appendix to Professor Orfila's General System of Toxicology, or Treatise on Mineral, Vegetable, and Animal Poisons, containing all the additional Matter relating to that Science, published by the Author in his last Work entitled "Lectures on Medical Jurisprudence," and thus rendering complete the former Treatise on Poisons; to which will be added Twenty-two coloured Engravings of poisonous Plants, Insects, &c.

XCI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 8. THE Royal Society commenced its sittings, the President Sir H. Davy, Bart., in the chair.

The Croonian Lecture on the Structure of the Eye, by Sir Everard Home, Bart., was read.

—— 15. The Bakerian Lecture on the Variation of the Compass, by Captain Sabine, R.A., was begun.

Nov. 22. Read a Paper on some Alvine Concretions; by J. G. Children, Esq.

—— 30. This being St. Andrew's day, the Anniversary of the Royal Society, the President announced that the Council had awarded the two Copley Medals; one to John Fred. Herschel, Esq., for his mathematical and optical Papers published in the Transactions; the other to Captain E. Sabine, R.A., for his Experiments on the Pendulum, and on Magnetism, made during two Expeditions of 1818 and 1819 to the Arctic Regions.

In his discourse, the learned President said, he was sure the Society would regard this decision of their Council with peculiar pleasure, as the labours for which the medals were awarded belonged to members of their own body, who were still actively engaged in the pursuit of science. In speaking of the papers of Mr. Herschel, Sir Humphry said, that he had not only distinguished himself by profound mathematical investigations, but had likewise made applications of the science of Quantity to physical researches of considerable extent and importance, proving himself as an analyst worthy to be associated with a Brinkley, an Ivory, a Woodhouse, and a Young, who in late times have redeemed the character of British Mathematics; entering those noble paths of investigation opened by the genius of Newton, and too long travelled in almost exclusively by illustrious foreigners. In Physical inquiry, he had, by his optical papers, added to the obligations already owing to the name of Herschel, in every thing connected with modern astronomy and the knowledge of the celestial spaces. The President then proceeded to point out at considerable length the object and nature of his researches, and gave an analysis of his papers. In delivering the medal to Mr. Herschel, the President begged him to receive it as a mark of respect of the Royal Society, and to preserve it as a pledge of future labours in their cause and that of science. He exhorted Mr. Herschel to employ his various talents with the same industry and zeal in the progress, as he had shown in the commencement of his career; and to recollect, that no pursuits were more useful, more dignified, and more honourable, at all periods of life. Of this he had a striking example in his illustrious father, who, full of years and of glory, must, he said, view his exertions with infinite delight, and, looking forward to the time when his own imperishable name would be recorded in the same annals of philosophy with that of his son, must enjoy as it were by anticipation a double immortality. In discussing Captain Sabine's labours, the President paid many compliments to the manner in which the Arctic expeditions had been planned and conducted. Active courage, he said, was so innate in the British character, that it hardly required praise: but there was a fortitude in meeting danger and difficulty, and a steadiness and patience in bearing privations, which demanded the highest commendation. These had been shown in a remarkable manner by Capt. Sabine, who in the Polar ice, and almost in darkness, had conducted his observations with as much precision as if he had enjoyed the repose and conveniences of the happiest climate and situation.

Sir Humphry entered into a historical view of the progress of experiments made upon the pendulum; and did ample justice to the accuracy and beauty of Captain Kater's invention, which Captain Sabine employed in his experiments: these seem to have been conducted with equal address and industry, and give a compression of about $\frac{1}{315}$ for the polar diameter of the earth.

"Captain Sabine (Sir Humphry said) is not here to receive the mark of your approbation; for, after braving the long night and almost perpetual winter of the Pole, he is now gone, with the same laudable object, to expose himself to the burning sunshine and perpetual summer of the equator." After expressing his warm hopes that he would return from these new and dangerous expeditions, after having accomplished all his objects, Sir Humphry presented the medal to Mr. Sabine, assuring him of the deep interest taken by the Royal Society in his brother's pursuits and success. The President dwelt in [illegible]ng terms on his disinterestedness and genuine love of science, which he said made him consider the good opinion of the Royal Society as the highest reward he could receive for his scientific labours. He had no doubt, he said, that if it pleased Providence to grant him health and a safe return, he would not only establish fresh claims to their admiration, but likewise to those of his countrymen who were lovers either of useful science or of bold and hardy enterprise.

The Society then proceeded to the Election of a Council and Officers for the ensuing year; when, on examining the lists, it appeared that the following gentlemen were elected:

Of the Old Council.—Sir Humphry Davy, Bart., W. T. Brande, Esq., the Lord Bishop of Carlisle, Taylor Combe, Esq., Davies Gilbert, Esq., Charles Hatchett, Esq., J. F. W. Herschel, Esq., Sir Everard Home, Bart., John Pond, Esq., Wm. Hyde Wollaston, M.D., Thomas Young, M.D.

Of the New Council.—The Earl of Aberdeen, Matthew Baillie, M.D., John Barrow, Esq., B. C. Brodie, Esq., Wm. Hamilton, Esq., James Ivory, Esq., The Marquess of Lansdown, Alexander Marcet, M.D., [illegible] Murdoch, Esq., Sir Robt. Seppings, Knt.

Officers.—President, Sir Humphry Davy, Bart. LL.D.

Treasurer.—Davies Gilbert, Esq.

Secretaries.—Wm. Thos. Brande and Taylor Combe, Esqrs.

ASTRO-

ASTRONOMICAL SOCIETY OF LONDON.

Dec. 14. A Letter was read from Captain Basil Hall, dated Valparaiso, May 13, 1821, giving an account of a comet which had recently appeared in that quarter; and communicating a number of observations of the same: whence its orbit may be deduced when they are published. At the conclusion of his letter, Captain Hall mentions a fact which either is not generally known, or does not appear to have been sufficiently attended to: which is, that occultations of the stars by the moon are *easily discernible at sea;* and that he himself has made several observations of this kind, and regrets that they are not announced in any ephemeris. As this mode of determining the longitude is much preferable to that by the eclipses of Jupiter's satellites, there will be no occasion for *marine chairs*, or any other contrivance for observing them.

A Letter was communicated, by the American Minister, from Mr. Lambert, of Washington, containing some tables for determining the moon's semidiameter in time, or the interval of passage from either limb to the centre, when passing the meridian. The author conceives this mode of determining the longitude to possess many advantages.

A Letter was also received from the Rev. M. Ward, relative to an opinion he had formed, that the western cavities of the moon would reflect sufficient light to produce a phosphoric appearance, similar to what he had before observed in May last. He was confirmed in his opinion by an observation of the spot *Manilius*, for the space of [illegible] minutes, on November 20th; and by a faint appearance, precisely in the situation of *Menelaus*. Mr. Ward conceives that each spot has not only a particular month, but also a particular day in each lunation, on which it is most favourably situated for such observations.

The next meeting of the Society will be on January 11, 1822.

XCII. *Intelligence and Miscellaneous Articles.*

TO OUR READERS.

WITH the present Number is given a portrait of the EDITOR. It is but proper that he should state that the plate was not engraved at his own expense. Mr. Henry Fisher, [illegible] proprietor of the Caxton Press some time ago requested [illegible] the loan of a portrait, painted by Mr. Fraser, for [illegible] having it engraved for the [illegible] and in return [illegible]

[illegible] what

what he was pleased to call the favour, Mr. Fisher, unsolicited and unexpected, sent the Editor a number of impressions sufficient for the Philosophical Magazine. Under such circumstances, he thought he would not withhold them from the numerous friends who have for so many years (now almost a quarter of a century) patronized this publication.

POPULATION OF FRANCE.

In the year 1820, the population of the eighty-six departments of which the kingdom of France, according to the treaties of 1814 and 1815, now consists, was 30,407,907 individuals. In the year 1819 there were 990,023 births, and 786,338 deaths; making an excess of births amounting to 203,685.

ANTIQUITIES OF NUBIA.

M. Jomard, of the French Institute, has just received a letter from M. Caillaud, dated the 5th of May, from Assour, a village about a day's journey from Chendy, in Nubia, in the kingdom of Sennaar, in which that traveller communicates his latest discoveries. At a short distance to the south of the confluence of the Atbara *, the ancient Astaboras, and four days' journey from Barbas, he found the ruins of a great town, with a temple and 40 pyramids still standing, and 40 others in ruins. The bases of the largest of these pyramids are about 62 feet, and their height 77, and on one of the sides of each is a small temple ornamented inside and outside with hieroglyphic characters; two of those temples are arched, and the arches are decorated with hieroglyphic emblems, and with key-stones and ribs like ours. The traveller has ascertained that those temples are of the same [illegible] as the Pyramids. All the materials are of freestone, like the [illegible] on which they are built. Ismail Pasha, who commands the military expedition into Abyssinia, permitted M. Cailliaud to open one of these Pyramids; some Greek letters were found in another of them. The site of the temple and the ruined town is about a league and a half from the Nile, and most of the pyramids are a league further, the same as at Memphis. Bruce must have passed two leagues only to the east without suspecting their existence. An avenue of Sphinxes, in the shape of rams, 262 feet long, leads to the temple, and the wall which incloses it is 426 feet round. The island of Curgos, mentioned by Bruce, is to the south of Assour, and contains no monuments. M. Jomard is of opinion [illegible] the great ruins near Assour are those of Meroe; their latitude, about 16 degrees 50 minutes, agrees with that of Meroe,

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* [illegible] Antiquities of Mount Barkal, near a place called Merawe, are about 90 [illegible] below, and very far from the confluence of the Atbara, which formed the Island Meroe.

as given by Strabo and Eratosthenes. The positions laid down by Bruce, in his map, are tolerably accurate, but he has traced the limits of the ruins too much to the south. M. Cailliaud proposed to remain during the rainy season at Sennaar, with the expedition, to take up his residence in the Fazuelo, and to proceed afterwards up the *Bahr-el-Abiad*, or the White River, which he will ascend to a certain distance, in order to procure information respecting the course of the Niger. The thermometer was constantly during the month of April as high as 45 degrees and upwards, and even as high as 48 degrees (43 degrees of Reaumur, exposed no doubt to the sun). M. Cailliaud could not discover any remains of the tradition of Queen Candace, whose dynasty, according to Bruce, were in his time still on the throne of Chendy. For a long time our traveller has not taken the meridian altitudes of the sun, which is too close to the zenith, and he can only determine the latitudes of places by means of the moon and stars.

SIERRA LEONE NEGOTIATION.

Mr. O'Byrne, sent from Sierra Leone to establish a commercial intercourse with certain African Chiefs of the interior, has entered the country of Limba, by Laiah, a city distant about seven leagues from the river which forms the boundary of the country of Timmani. His reception was very favourable with all the chiefs, one of whom, of Port Logo, accompanied him to Woulla, and sent his brother with him to Koukouna. From this last place he advanced to the frontiers of Foulah, the chiefs of which agreed, in a palaver, to open a commercial correspondence with Sierra Leone. It appears that Dacho, King of Sego, was sending a party to the Governor of Sierra Leone to invite the Whites to visit and trade in his kingdom, and had recommended to the King of Timbo to provide for the security of such strangers as should proceed to Bambarra through the country of Foulah Yallon. This rendered unnecessary the further advance of Mr. O'Byrne.

AUSTRALASIA.

Accounts have been received here by the ship Dick, lately arrived from India, from His Majesty's brig Bathurst, Capt. King, employed in examining the unexplored Coast of Australasia, dated off Goulburn Island, on the North Coast of New Holland, the 6th of July last; the ship Dick and brig St. Antonio then in company, which the Bathurst had piloted from Port Jackson, on their way to India, through a most intricate and dangerous navigation, in which the latter lost two anchors. At the date of the letter they had been out six weeks from Port Jackson; three weeks whereof they had been sailing among coral reefs of frightful

ful appearance, and were obliged to anchor every night wherever they could find shelter, not daring to proceed after sunset, having had many narrow escapes even in the day-light, but were at the period before mentioned entirely clear of that dreadful coast. They lost their two anchors and cables under Cairncross Island at 11 P. M. on the 30th of June, and nothing but the tide, which fortunately set to windward, kept them clear of the dangers which surrounded them on every side; and the weather being so exceedingly bad at the time, their escape was a miracle. Mr. Perceval Baskerville, a midshipman of the Bathurst, and a native of Plymouth, was sent on shore with a party on the easternmost island of Flinders's Group, for the purpose of picking up any part of the wreck of the ship Frederick, which had been lost there; when they were encountered by a large party of the natives, who commenced a horrible shout, which proved the signal to engage, and they commenced by throwing a shower of spears with great agility, by which two of the party were wounded. The Bathurst's people, being unarmed, could make no other resistance than by defending themselves with stones, while a party of them were immediately dispatched in the boat, in order to procure fire-arms from the ship. The natives, seeing the transaction, took the opportunity, while the boat was absent, to attack those left on shore more violently, and Mr. Baskerville and his little party were surrounded and made prisoners. However, no attempt was made to take their lives after the capture; and on the return of the boat, through artifice, they again joined their comrades. But shortly afterwards the natives came down in great numbers, and again attacked the party, who, being now armed, gave them a volley that occasioned them to scamper off in all directions, leaving two on the ground wounded; but they soon after got up and escaped, and no others appeared while the Bathurst remained there.—*Plymouth Telegraph.*

NEW SOUTH WALES.

Letters are in town from Port Jackson, to the middle of June, by the Skelton. at which period the prospects of the territory continued to bear the same favourable aspect they have long assumed.

Mr. Throsby had returned on the 20th of April from an excursion into the country to the southward of Lake George. The persevering efforts of this gentleman in exploring the interior of this territory have often attracted the public attention; and have contributed in a very eminent degree to open to the colonists a large tract of land, that now affords abundant pasturage to a considerable number of cattle and sheep, and has much relieved the exhausted and overstocked grazing-grounds in the early-settled parts of the colony. In his late journey, Mr. Throsby fell in with three very considerable rivers, or streams of water, apparently originating in the high lands at the back of Jarvis and Bateman's Bay, and taking a westerly course. The country was of various description, but containing a good quantity of open forests and plains, very abundant in water, and affording good pasturage. Limestone was found in great plenty, and specimens had been brought in.

In Mr. Throsby's letter, detailing the tour, he says, "I admit the great extent of country through which these rivers appear to run, places it far beyond my power to determine their termination; yet I still hope they will be ultimately found to communicate with the sea, but most certainly not on the eastern coast.

"I am happy to report, that the country in general is superior to that which we passed through when with His Excellency the Governor in November last. It is perfectly sound, well watered, with extensive meadows of rich land on either side of the rivers; contains very fine limestone in quantities perfectly inexhaustible, slate, sand stone, and granite fit for building, with sufficient timber for every useful purpose; and, from the appearance of the country, an unbounded extent to the westward.

"The approach from Lake George is in no part more difficult than the track the Governor's carriage and carts passed between Lake Bathurst and Lake George on his late tour; nor do the very high mountains to the south-east present that prospect of extreme barrenness which the mountains bounding this part of the colony do; the whole being thinly timbered, with a pleasant appearance of verdure between the trees."

NATURAL HISTORY.

Whales.—The Aleutians count seven species of whales, tl most of which are probably unknown to natural history. One of these species is a rapacious ani[illegible] which is well known not to be the case with other whales, [illegible] they have no teeth. It devours every thing it can catch, and [illegible] pursues the Aleutians, whose little baydaus, if it is able to overtake them, it upsets with one blow of its tail. It is said that a baydau, with 24 oars and 30 men, was lately destroyed by the blow of such a monster, near Oonalashka. The Russians and Aleutians relate, that if a piece of the blubber of this animal is swallowed, it has the property of immediately passing through the body undigested.

Sea Serpent.—M. Kriukof's description of a sea-animal, which pursued him at Behring's Island, where he had gone for the purpose of hunting, is very remarkable. Several Aleutians affirm they have often seen this animal. It is of the shape of the red serpent, and immensely long; the head resembles that of the sea-lion, and two disproportionately large eyes give it a frightful appearance. "It was very fortunate for us," said Kriukof, "that we were so near land, or else the monster would have swallowed us: it stretched its head far above the water, looked about for prey, and vanished. The head soon appeared again, and that considerably nearer: we rowed with all our might, and were very happy to have reached the shore before the serpent. The sea-lions were so terrified at the sight, that some rushed into the water, and others hid themselves on the shore. The sea often throws up pieces of flesh, which, according to opinion, is that of this serpent, which no animal, not even the raven, will touch. Some Aleutians, who had once tasted some of it suddenly died. If a sea-serpent really has been seen on the coast of North America, it may have been one of this frightful species.

Gigantic Polypus.—The Aleutians also relate stories of a gigantic polypus. It has happened, that a polypus has thrown its long arms, which are twice as thick as a strong man's arm, round the baydau of an Aleutian, and would have carried it into the abyss, if the Aleutian had not had the presence of mind to cut through with his knife the fleshy arm of the polypus, which was furnished with large suckers. The polypus remains with his body fast at the bottom of the sea, and generally chooses a place from which it can reach the surface with its arms. The last accident happened in the passage which is found by the southern point of the island of Oomnack, and the little island lying near it.—*Kotzebue's Voyage*, ii. 183.

A Female Shark.—The ship Brailsford, on her passage from Bombay to England, in latitude 29. 26. S. long. 40. 2. E. caught a large blue female shark 12 feet long, on opening which there were found no less than 77 young ones alive, each about a foot long, and weighing from one half to three quarters of a pound.

FOSSILS.

In the beginning of November, Mr. Mantell discovered in the chalk near Lewes, three vertebræ of the celebrated fossil animal of Maestricht. This is the first instance of the remains of that oviparous quadruped being found in this country, or in any part of the Continent, except in St. Peter's Mountain near Maestricht.

SUPERB MUMMY.

A Danish family, desirous of purchasing a beautiful mummy for one of the museums in Copenhagen, wrote to M. Dumrecher, Danish Consul at Alexandria; who, assisted by M. Tedenat, the French Consul, procured an intelligent man to set out for Upper Egypt, with a firman from the Pasha, to search the tombs of the ancient kings. For the greater dispatch, they employed two different parties of the natives, from Longsor and from Karnack. The former were the most fortunate, discovering a tomb that had never been opened. and where they found, on the third day, a mummy with five cases; they asked for this 6000 piastres of Egypt (133*l.*), which was paid them. The fellahs of Karnack, thus disappointed, and having had three days toil for nothing, had warm disputes with those of Longsor; and mischievous consequences might have ensued, as their villagers took a part in the quarrel, if the possessor of the mummy had not given 1000 piastres (22*l.*) extra to the Arabs of Karnack, to whom also some participation was made by those of Longsor. This mummy is the most superb and beautiful of all that have hitherto been discovered. To judge of it from the ornaments in relief, which decorate the cases, and especially one whereon gold has been lavished, from the rich style of the amulets, from the largeness of the papyrus, and all the hieroglyphical embellishments about the body, it must have been that of some Egyptian king or prince. This conjecture is coroborated by the number of cases, as the mummies of the greatest persons in general have only three.

METEORIC STONES.

M. Fleurian de Bellevue, in a paper read last year before the Academy of Sciences, on meteoric stones, and particularly on those which fell near Jonzac, in the department of Charente, draws the following conclusions respecting these bodies:

1. The appearances presented by the crust of meteorolites seem to prove that their surface has been fused whilst rapidly traversing the flame of the meteor, and rapidly solidified into a vitreous state on leaving that flame.

2. They prove that in the first moments, the movement of the meteorolites was simple; that is, that they did not turn round on their own axis whilst those two effects took place.

3. That the impulse each meteorolite has received has almost always been perpendicular to its largest face.

4. That the largest face is almost always more or less convex.

5. Our meteorolites (those of Jonzac) offer new proofs of the pre-existence of a solid nucleus to bolides or meteors.

6. This nucleus could not contain the combustible matter which produces the inflammation of the meteor.

7. It cannot have suffered fusion during the appearance of the phenomena.

8. The gaseous matter which surrounds this nucleus is dissipated without producing any solid residuum. No trace of this matter appears ever to exist in the crust of the meteorolites.

9. Meteorolites are fragments of those nuclei which have not

been altered in their nature, but simply vitrified at their surfaces.

10. Many of the irregular forms which these fragments present may be referred to determined geometric forms.

11. Those latter forms are the consequence of the rapid action of a violent fire, according to a law of the movement of heat in solid bodies, discovered by M. Emer.—*Quarterly Journal.*

METEOR.

A most beautiful meteorological phenomenon was witnessed at Brighton late on Sunday night, the 2d of December. It was a swift shooting luminous ball, which continued perfect a few seconds, and then assuming the appearance of a fine large sky rocket, became gradually dissolved amidst a wide-spreading shower of splendid sparkling fire.

THE SHOWER OF SNAILS.

To Dr. Tilloch.

Bristol, Nov. 19, 1821.

SIR,—There appeared two extracts from the Bristol and Gloucester Newspapers in your October Number, respecting a shower of snails said to have fallen at Tockington near Bristol.

Having heard such a report at the time, I was anxious to examine into the truth of it, particularly as it was represented to have had some sort of connexion with the curious azure-blue appearance of the sun: accordingly I went over to Tockington with two friends, expecting to find an immense quantity of periwinkles deposited there by a water spout—two or three inches deep at least—as it was stated that the farmers had carried them away by waggon loads to manure their land with. You may imagine our astonishment, when upon asking our guide how much further we had to go, *he told us we were on the spot.* Upon an attentive examination, I could perceive there were many small snails there; but I do not believe that I could in any part of the field have covered more than a dozen with one of my hands; almost as many appeared in a field on the opposite side of the turnpike road, *but not one upon the road itself.* I inquired whether many had been seen there in former years, and heard that almost as many were there last year. I shall not trouble you with the answers to every question asked, but it turned out very similar to the well known story of the three black crows. A man had walked over the field in the morning, and observed nothing particular; but on returning in the evening was struck (not with a snail) but with the *appearance* of a great number of snails, and jocosely observed "a body would think it had rained snails." This being repeated with a little addition to an old granny in the village, she remarked she did not doubt it; indeed she thought she "felt something fall on her umbrella as she went over the field in the evening before." On the arrival of this story in Bristol, *only eight miles*, it amounted to this: A dreadful storm had happened which drove every person from a fair held there; and that when the morning came they found twenty-eight acres of land covered six or seven inches deep with snails, which had fallen with such force as to beat holes in people's umbrellas!!! Some of them were brought here, and sold on the Exchange at a halfpenny each, and I am given to understand that a respectable tradesman in Bristol had some of them boiled, and ate them as *shell-fish*.

If you think it necessary that such a statement should be corrected, and that this answers the purpose, you are welcome to print it. I am, sir, yours, &c.

WILLIAM HERAPATH.

EARTHQUAKES.

On Monday the 15th of October, at an early hour in the morning, an earthquake was felt over the island of Bute. At Rothsay and in its vicinity, a tremulous motion was experienced, which lasted a few seconds, and is said by some to have been accompanied by a sound similar to that of the distant rolling of carriage wheels. It was also felt at Greenock, though so slightly as not to excite speculation till corroborated by the above information from Bute.

On Tuesday the 23rd of October, at 3 P. M., a shock of an earthquake, more severe and alarming than any previously experienced in that quarter, was felt at Comrie. The noise which accompanied the shock was sensibly felt by many persons at the distance of nearly 20 miles in a southerly direction. A gentleman of Stirling, who had been walking that day along the Teath with some friends, says that it took place about three o'clock in the afternoon. The noise, which was accompanied with a slight tremulous motion, is described as like the rolling of distant thunder, but was at the same time so distinct, and sensibly felt by all of them, that each instantly declared it to be the effect of an earthquake. The spot on which they were then standing is only about three miles north-west of Stirling. Similar effects were felt at Blacklord and neighbourhood.

EXTRAORDINARY SHIPWRECK.

On the 19th of November, 1820, in lat. 47° S., long. 118° W. the American South Sea whaler Essex, of 250 tons, G. Pollard master, from Nantucket, met with the following singular accident. On that day the vessel was among whales, and three boats were lowered down: the mate's boat got stove, and had returned to the ship to be repaired. Shortly after a whale of the largest class struck the ship, and knocked part of the false keel off just abreast of the main channels. The animal then remained for some time along-side, endeavouring, but in vain, to clasp the ship with her jaws: she then returned, went round the stern, came on the other side, and went away a-head about a quarter of a mile, when suddenly turning, she came at the ship [illegible] tremendous velocity, head on. The vessel was going [illegible] of five knots; but such was the force when she struck [illegible] which was under the cat-head, that the vessel had [illegible] the rate of three or four knots; in consequence of which [illegible] rushed into the cabin windows, every man on deck was [illegible] down, and the bows being stove completely in, the vessel [illegible] and went on her beam ends. By cutting away the masts, [illegible] vessel righted; the upper deck was then scuttled; and [illegible] water and bread were procured for the two boats, in which [illegible] captain and crew, in expectation of falling in with some [illegible] remained three days by the wreck, but were compelled at [illegible] to abandon it. On the 20th December they made Ducie's [illegible] at which place the boats remained one week; but the [illegible] affording hardly any nourishment, they resolved on venturing for the continent, leaving behind three men. The two boats, soon after leaving the island, parted. One of them, containing only three men, was picked up by an American whaler about 60 days after the wreck. The other, in which the captain was, was fallen in with, by another whaler 90 days from the time of their leaving the island. Only two of her crew then survived, and their account of their sufferings was dreadful in the extreme. From hunger they had been reduced to the painful necessity of killing and [illegible]vouring each other. Eight times lots had been drawn, and [illegible] human beings had been sacrificed to afford sustenance to [illegible] that remained; and on the day the ship encountered them, [illegible] captain and the boy had also drawn lots, and it had been [illegible] determined that the poor boy should die! But providentially [illegible] whaler hove in sight and took them in, and they were restored to existence. Captain Raine of the Surrey, having learnt this melancholy tale at Valparaiso, whence he was about to sail for [illegible] South Wales, resolved to make Ducie's Island on his way, [illegible]scue the three men left there, if still in existence. On nearing the island a gun was discharged, and shortly after the three [illegible] men were seen to issue forth from the woods. The boats were presently lowered, and the men with considerable difficulty, owing to a heavy surf, were got on board.

SOUTH AMERICAN BOTANY.

It was some time ago stated in the accounts received from New Granada, that the whole, or the greatest part, of the results of the botanical researches of the celebrated Mutis, carried on at the expense of the Spanish Government for more than forty years, in one of the finest regions of South America, had been recently destroyed amidst the conflicts of contending armies, and considerable regret was excited in the breasts of scientific men on account of so irreparable a loss. We have, however, the satisfaction to announce, that the whole, with the exception of a few indices and partial descriptive catalogues, have arrived safe at Madrid, and are now deposited at the Botanical Garden, in charge of Professor Gasca, who very kindly showed the series of drawings to a gentleman lately arrived from Spain. They were executed in the most beautiful style, on the spot, chiefly by South Americans, who, it is acknowledged, have a peculiar taste for design and painting, and they exceed 4000. The specimens were collected in wide and secluded districts, in a tropical clime, and all copied the moment each plant was gathered. This gives to the drawings a brilliancy and nature almost unequalled, and among them are some hundreds of plants never before known in Europe. The history of the *Cinchona*, or febrifuge bark, in a long series of drawings, embracing the genera and extensive varieties, is peculiarly fine. This valuable treasure fell into the hands of General Morillo when he entered Santa Fe, and he had the whole packed up and sent down to a shipping port, where the packages were embarked for Spain. The descriptive pieces were at the time left in the country, and consequently they are not lost. Owing to the distressed state of the finances in Spain, it may be many years before this collection, which no doubt stands unrivalled, can be laid before the public. We therefore take the liberty to suggest, that General Bolivar, and the Government over which he presides, in whatever arrangements they may hereafter make with the Ministers of Spain, respecting the acknowledgement of their independence, ought to stipulate for some plan for the publication of Mutis's labours. This is due to science in general, as well as to the memory of that distinguished botanist and his worthy coadjutors, some of whom, particularly the lamented Caldas, fell victims in that very contest which is now so near its close.

SOUTH AMERICAN POTATOE.

Some time last year, Mr. Thomas Lorimer, residing near Rockhall, the seat of Sir Robert Grierson, Bart., received from an acquaintance, a single potatoe which had been brought from

Spanish Town, South America. This potatoe he kept till spring, when cutting it in two, he planted the pieces at a trifling distance from one another, in a corner of his garden. These plants, or slips, speedily sprung up, and in due time put forth blooms and apples like any other potatoe; and there was nothing either in the colour or luxuriance of the shaws that excited particular notice; but on raising the said exotics, Mr. Lorimer found to his surprise, that they had produced no fewer than 41 potatoes, [illegible] of which are of an uncommon size. Two of the largest [illegible] were brought to this office a few days ago, one of which weighed 2lb. 2oz., and the other 1lb. 14oz. while both measured nearly 18 inches in circumference. From the size and appearance of the *thumping* roots, we were inclined to set them down as a species of the yam: but on this point Mr. L. completely undeceived us, by declaring that the residue of the produce, which cannot weigh much less than 30lbs., is rather of a round shape, and in other respects bears a pretty close resemblance to the common potatoe.—*Dumfries Courier.*

SPADE AND PLOUGH HUSBANDRY.

In the neighbourhood of Hamilton an experiment was made this year to try the difference between the spade and the plough. A field was taken, which was in beans last year, and oats the year before; two ridges were dug and two ploughed alternately, and the whole was sown on the same day; a part both on the ploughed and dug being drilled with the garden hoe; the whole was reaped the same day; and being thrashed out, the result was that the dug sown broadcast was to the ploughed sown broadcast as 55 to 42. The dug and drilled was as 20¼ to 12¼, upon the ploughed and drilled. The additional grain is not the only beneficial result gained by digging, as in this instance there was also a great deal more straw. The land is free of weeds, and will be more easily fallowed next year.

ANIMAL SAGACITY.

We do not think the records of *instinct* ever contained a more extraordinary instance than we are now about to relate, and for the truth whereof we pledge ourselves. A few days since, Mr. Joseph Lane, of Fascombe, in the parish of Ashelworth, in this county, on his return home, turned his horse into a field in which it had been accustomed to graze. A few days before this, the horse had been shod all-fours, but unluckily had been pinched in the shoeing of one foot. In the morning, Mr. Lane missed the horse, and caused an active search to be made in the vicinity, when the following singular circumstance transpired:—The animal,

mal, as it may be supposed, feeling lame, made his way out of the field by unhanging the gate with his mouth, and went straight to the same farrier's shop, a distance of a mile and a half. The farrier had no sooner opened his shed, than the horse, which had been evidently standing there some time, advanced to the forge, and held up his ailing foot. The farrier instantly began to examine the hoof, discovered the injury, took off the shoe, and replaced it more carefully; on which the horse immediately turned about, and set off at a merry pace for his well-known pasture. [illegible] Mr. Lane's servants were on the search, they chanced to [illegible] by the forge; and on mentioning their supposed loss, the [illegible] replied, "Oh! he has been here and shod and gone home again;" which on their return they found to be actually the case.—*Cheltenham Chronicle.*

MAGNETISM.

The *Prussian State Gazette* mentions a highly important discovery, which Dr. Seebeck had communicated to the Academy of Sciences at Berlin, in three different sittings. It was on the magnetic properties inherent in all metals and many earths, (and not in iron only as was supposed,) according to the difference of the degrees of heat. This discovery, it is added, opens an entirely new field in this department of natural philosophy, which may lead to interesting results with respect to hot springs, connected with the observations made by the Inspector of Mines (M. Von Trebra) and others, relative to the progressive increase of warmth in mines in proportion to their depths. According to M. Von Trebra's observations, the heat at the depth of 150 feet below the surface of the earth is one degree; at 300 feet deep, two degrees; at 600 feet four degrees, &c.

LETTER FROM MR. IVORY TO THE EDITOR.

SIR,—I find in your last Number, a letter from my late antagonist, which would not have required any immediate notice from me, if he had not said that his *veracity* was called in question. Now I am not conscious that I used any words or mode of expression that can be so interpreted; at least, I certainly never intended to insinuate any thing of the kind.

He also says that he was attacked upon his own ground; which is somewhat strange; for, on the other hand, I always thought that I was defending myself from an attack made upon me without any ground at all. I am, sir,

Your obedient servant,

Dec. 20, 1821. JAMES IVORY.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Parkin, of Skinner-street, Bishopsgate-street, London, merchant, for his improvements in printing.—Dated the 24th November 1821.—6 months allowed to enrol specification.

To William Baylis, junior, of Painswick, Gloucestershire, clothier, for his machine for washing and cleaning cloths.—27th November.—2 months.

To Thomas Motley, of the Strand, Middlesex, patent [illegible] maker, and brass founder, for his improvements in the construction of candlesticks or lamps, and in candles to be burnt therein.—27th November.—6 months.

To Robert Bell, of Newman-street, parish of St. Mary-le-bone, Middlesex, esq., for his improvement in the construction of certain descriptions of boats and barges.—5th December.—6 mo.

To Charles Broderip, of London, esq., now residing in Glasgow, for his various improvements in the construction of steam-engines.—5th December.—6 months.

To Henry Ricketts, of the Phœnix Glass-Works, Bristol, Somersetshire, glass-manufacturer, for his improvement in the art or method of making or manufacturing glass bottles; such as are used for wine, porter, beer or cyder.—5th December.—2 mo.

To William Warcup, of Dartford, Kent, engineer, for his improvements upon a machine for washing linen cloths, cotton cloths, or woollen cloths, whether in the shape of piece goods or of any article made up of linen cloth, cotton cloth, or woollen cloth.—10th December.—2 months.

To William Horrocks, of Portwood within Binnington, Chester, cotton manufacturer, for his improvement in the construction of looms for the weaving of cotton or linen cloth, by power commonly called Power looms.—14th December.—2 months.

To James Winter, of Stoke-under-Hamdom, Somerset, [illegible]tleman, for his improvements in a machine for sewing and pointing leather gloves with neatness much superior to that which is effected by manual labour.—19th December.—2 months.

To Samuel Brierley, of Salford, in the parish of Manchester, Lancaster, dyer, for his improved method of preparing raw silk and cleansing the same for the purpose of dyeing and manufacturing.—19th December.—2 months.

To John Gladstone, engineer, and millwright, of Castle Douglas, in the Stewartry of Kirkcudbright, county of Galloway, North Britain, for his improvements in the construction of steam vessels by the application of steam or other power.—20th December.—6 months.

BAROMETRIC OBSERVATIONS.

Crumpsall, Lancashire, Dec. 12, 1821.

SIR,—The following observations were taken on the 12th of November, and the 10th of December.

Your obedient servant,

To Dr. Tilloch. JOHN BLACKWALL.

CRUMPSALL.

	Bar.	Ther. att.	Ther. det.	Wind.	Weather.
1821. A.M					
Nov. 12th 8h.	29 380	47°	45 5	S W. brisk.	Foggy
9	29 400	48	46 3	S W. do.	Foggy, with faint [sunsh.
10	29.410	48	48	S W. do.	Sunshine through
11	29.430	50·5	50·5	S W. high.	Do [clouds.
12	29 445	51	51	S W. do.	Do.
P.M. 1	29 457	52 2	52 3	W. do.	Do.
Dec. 10th, A.M.					
8	29·500	53	53 5	S boisterous	Cloudy.
9	29 500	53	53 5	S do	Do
10	29 500	53	54	S do	A little rain.
11	29 480	53	54 5	S do.	Cloudy, with faint
12	29 460	53 5	55	S do	Do. [sunsh.
P M. 1	29 440	53 5	55	S do	A little rain.

MANCHESTER.

1821. A.M.	Bar.	Ther. att	Ther. det.	Wind.	Weather.
Nov. 12th,					
8h	29 600	54°	[illegible]	S E brisk	Foggy.
9	29.620	54	46	S E do	Fine.
10	29.640	54	47 5	S E do	Do.
11	29.645	55	49 5	S W. high	Do.
12	29 650	55	51 5	S W. brisk.	Do
P M. 1	29 660	56 5	53	S W. do	Do.
Dec. 10th, A.M					
8	29 700	55	54	S.W do.	Do.
9	29 680	55 5	54 5	S.W. do.	Do.
10	29 680	56	56	S.S.E. high.	Cloudy.
11	29 675	56·5	57	S S.E. do.	Do.
12	29 660	57	57·5	S.S.E. do.	Do.
P M 1	29 600	58	58	S. do.	Do.

* * *

Pocklington, Yorkshire, Nov. 17, 1821.

SIR,—The following observations I made at this place on the 12th of this month, at the hours given below.

I am, sir, yours truly,

WILLIAM ROGERSON, jun.

Clock.